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Essays in Corporate and Household Finance

Abstract

This dissertation studies two questions in corporate and household finance: 1) Can fiscal stimulus policies targeted at firms incentivize investment and improve firm financial conditions?, and 2) Do the effects of consumer credit on household well-being vary with differing economic states?

In the first chapter, I examine the effects of a countercyclical fiscal policy that relaxed firm financial constraints by giving firms additional tax refunds. To estimate the policy's impact, I use a regression kink design strategy that takes advantage of a discontinuity in the slope of the tax refund formula. I find that after passage of the 2002 policy, firms allocated \$0.40 of every tax refund dollar to investment. After passage of the 2009 policy, in contrast, firms used the refunds to increase cash holdings (\$0.96 of every refund dollar) before paying down debt in the following year. While the policy had no discernable effect on investment in the most recent recessionary period, it did reduce firms' bankruptcy risk and the probability of a future credit rating downgrade.

In the second chapter, I provide empirical evidence that access to credit has state-dependent effects on household material well-being, even within the market for one credit product--in my case, payday lending. Using detailed data on household location and consumption patterns, I show that access to payday credit lowers material well-being in "normal" states of the world. Loan access results in substantial declines in nondurable goods spending overall and in housing-related spending particularly. Following temporary negative shocks, however--extreme weather events like hurricanes and blizzards--I show that payday loan access helps households smooth consumption and improves material well-being. After extreme weather events, loan access mitigates declines in spending on food, mortgage payments, and home repairs.

Degree Type

Dissertation

Degree Name

Doctor of Philosophy (PhD)

Graduate Group

Finance

First Advisor

David K. Musto

Keywords

Consumer Credit, Corporate Finance, Fiscal Policy, Household Finance, Payday Lending, Taxation

Subject Categories

Finance and Financial Management

ESSAYS IN CORPORATE AND HOUSEHOLD FINANCE

Christine Louise Dobridge

A DISSERTATION

in

Finance

For the Graduate Group in Managerial Science and Applied Economics

Presented to the Faculties of the University of Pennsylvania

in

Partial Fulfillment of the Requirements for the

Degree of Doctor of Philosophy

2015

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*To my husband Paul, for his unwavering support, and to my parents, Jane and Michael,
and my grandfather Guy, who taught me all that hard work can bring.*

ACKNOWLEDGMENT

Though there's one name on the title page of this dissertation, I would never have completed it without the guidance and support of so many. I am forever grateful to:

My committee: Andy Abel, Jennifer Blouin, Todd Gormley, David Musto, and Nick Roussanov. I benefited enormously from your extensive advice throughout this process. Thank you for being role models for the type of researcher I hope to become. I am also grateful for many helpful discussions with Alex Edmans, Erik Gilje, Vincent Glode, Joao Gomes, Mark Jenkins, Karen Lewis, Michelle Lowry, Stephanie Sikes, Luke Taylor and Jeremy Tobacman.

The economics faculty at Wellesley College, especially Corri Taylor and Brock Blomberg, for the care and time they devote to their teaching and their students. They pointed me down this crazy path, as they have so many women before me. You said you thought I could do it, and I did!

My Wellesley swim coach, Bonnie Dix. Swimming for the Blue, I learned that with perseverance and determination, I can accomplish more than I ever would have thought.

My bosses at the CEA, Deutsche Bank, and the Treasury: Steve Braun, Peter Hooper and Phillip Swagel. You helped me discover my great—and somewhat inexplicable—love of data and my interest in macroeconomic and financial conditions as well as economic policy issues. Without your influence, I certainly wouldn't be here.

My classmates, for all their help and for a lot of laughs along the way, especially Jillian Popadak, Ian Appel, Gill Segal, Sang Seo and Jess Jeffers. Many thanks to Terrence Blackburne, Peter Blair, Adam Bloomfield, Yasser Boualam, Anna Cororaton, Aycan Corum, Matt Denes, Michael Lee, Andrew MacKinlay, William Mann, Louis Mao, Thien Nguyen, Ryan Peters, Jessica Tarica, Jerry Tsai, Dieter VanWallegghem, Colin Ward, Andy Wu, Ram Yamarchy and Yiwei Zhang as well.

The administrative, facilities and service staff at Wharton, especially Susan, Britton, Andrea, Michelle, Liz, Janet, Pat, Lisa, Frank, Donald and Kristen. You were always a bright spot in my day and please know I appreciate all you do.

Finally, I thank my friends and family who have always, always, always believed in me: Paul, Jane, Michael, Lauren, Susan, J.R., Cam, Barb, Steve, Shannon, Mike, Tom, Jen, Mark, Sue, Cathy, Dave, Sarah, Tim, Diana, Mike, Brad, Tim, Bridget, Anthony, Gretchen, Jason, Karen, Eric, Abbie, Joe, Erika, Josh, Annie, Whitney, Jess, Meg, Sarah, Sumana, Sarah, Jen, Cat, Alex, Priscila, Carl and Nellie. I couldn't have done it without you.

Thank you.

ABSTRACT

ESSAYS IN CORPORATE AND HOUSEHOLD FINANCE

Christine L. Dobridge

David Musto

This dissertation studies two questions in corporate and household finance: 1) Can fiscal stimulus policies targeted at firms incentivize investment and improve firm financial conditions?, and 2) Do the effects of consumer credit on household well-being vary with differing economic states?

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In the second chapter, I provide empirical evidence that access to credit has state-dependent effects on household material well-being, even within the market for one credit product—in my case, payday lending. Using detailed data on household location and consumption patterns, I show that access to payday credit lowers material well-being in “normal” states of the world. Loan access results in substantial declines in nondurable goods spending overall and in housing-related spending particularly. Following temporary negative shocks, however—extreme weather events like hurricanes and blizzards—I show that payday loan access helps households smooth consumption and improves material well-being. After extreme weather events, loan access mitigates declines in spending on food, mortgage payments, and home repairs.

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CHAPTER 1: Fiscal Stimulus and Firms: A Tale of Two Recessions

1.1 Introduction

In an attempt to stem severe declines in employment and output during the 2007-2009 recession and to spur a recovery, the U.S. government enacted an unprecedented level of fiscal stimulus—over \$1.5 trillion.¹ A number of stimulus provisions were targeted directly at the corporate sector with the goals of increasing investment and reducing firm financial distress. We have relatively scarce empirical evidence on the effects of firm fiscal stimulus, however, and from a theoretical perspective, the effects of these policies remain unclear. This is in large part because the measures were implemented in poor economic conditions when firm investment opportunities may have been weak. In this paper, I study the effects of a fiscal stimulus policy that gave firms additional tax refunds at the end of the past two recessions. I ask two questions: (1) What do firms do with fiscal stimulus funds?, and (2) does fiscal stimulus improve firm financial conditions more broadly?

The policy I study is the five-year carryback of net operating losses. Under the U.S. tax code, any firm experiencing a net operating loss (NOL) in a particular tax year can receive a refund for taxes paid in the previous two years—called an NOL “carryback” deduction. In 2002 and 2009, Congress expanded the carryback window from two years to five years to give additional tax refunds to firms. The policy was essentially an intertemporal transfer of tax benefits, not a cash windfall. A firm with an NOL also has the option to carry the NOL forward for 20 years to offset future taxable income—called an NOL “carryforward” deduction. Under the five-year carryback

¹ Major packages passed by the U.S. Congress included a \$150 billion package in February 2008, an \$830 billion package in February 2009, a \$45 billion package in November 2009, and an \$860 billion package in December 2010. For information on the cost estimates of provisions in these packages, see the following Congressional Budget Office publications: “H.R. 5140, Economic Stimulus Act of 2008: Cost Estimate” (February 2008), “H.R. 1, American Recovery and Reinvestment Act: Cost Estimate for the Conference Agreement for H.R. 1” (February 2009), “H.R. 3548, Worker, Homeownership, and Business Assistance Act of 2009: Cost Estimate” (November 2009), “H.R. 4853, Tax Relief, Unemployment Insurance Reauthorization, and Job Creation Act of 2010: Cost Estimate” (December 2010), and “Estimated Impact of the American Recovery and Reinvestment Act on Employment and Economic Output in 2013” (February 2014).

extension policy, most firms that received tax refunds would have expected to pay higher taxes in the future due to the reduction in NOL carryforwards available to offset future taxable income.²

Estimating the economic effects of fiscal stimulus is challenging due to difficulties disentangling the effect of the policy from other economic factors influencing firm behavior at the time. In particular, the empirical challenge in analyzing the five-year carryback extension policy is a potential endogeneity concern arising from omitted variable and simultaneity issues. Only firms with losses were eligible to receive refunds, and the firms receiving the largest refunds were those with the largest losses during the recession and the largest profits during the prior expansion (e.g., homebuilders in the 2009 policy period). Whether a firm received a tax refund and the size of a firm's tax refund could have been correlated, therefore, with other factors driving firm financial policies and performance, such as unobserved investment opportunities or managerial quality. In addition, firms had incentives to engage in tax planning to maximize their refunds, leading to the concern that managers may have chosen the tax refund size based on a desire to allocate the funds to a specific purpose.

I overcome these challenges by using a regression kink design (RKD) strategy. RKD has similar features to regression discontinuity design (RDD), but instead of exploiting variation around a discontinuity in the *level* of a policy variable of interest as in RDD, it exploits variation around a discontinuity in the *slope* of a policy variable (a “kink” in the variable). In my setting, a discontinuity in slope arises from the statutory formula that determines the size of a firm's tax refund. Under U.S. tax law, a firm's tax refund is based on its current losses and previous-years' profits. Firms can only offset past tax liability up to the point where previous-years' taxable income is equal to current losses. For example, if taxable income in the past five years is larger than current losses, a firm cannot receive any additional refund beyond the value based on current losses—the firm already maximized its tax refund at this point. This statutory requirement

² The Joint Committee on Taxation (JCT) estimated that in the 2002 policy period, the IRS would issue about \$15 billion in additional refunds to firms, but that the 10-year revenue cost would only be about \$2 billion. In the 2009 period, the JCT estimated the IRS would issue about \$38 billion in additional refunds with a 10-year cost of about \$11 billion.

introduces a kink in the slope of the tax refund formula at the point where previous-years' taxable income equals current losses. The RKD identification strategy relies on assumptions that are similar to RDD: that the number of firms is distributed smoothly around the kink point and that pre-determined firm characteristics evolve smoothly around the kink point (Card, Lee, Pei, and Weber, 2012). I present evidence that these assumptions appear to hold in my setting.

Policymakers specified two main goals when implementing the five-year carryback policy: to boost economic growth by increasing investment and employment, and to help firms smooth income and mitigate financial distress.³ While the policy goals were clear, whether giving firms tax refunds is effective in achieving these goals is an open question from a theoretical perspective. To begin, the policy acted as an intertemporal transfer of tax benefits as discussed above—i.e., a temporary cash flow change. With unrestricted access to financial markets, short-term changes in cash flow should not affect investment or performance because firms should already be optimizing their investment and financing policies.⁴

The policy did provide firms with liquidity in the short term, however, and theory suggests firms may have had incentives to use the refunds to increase cash holdings or reduce debt. Firms facing high idiosyncratic risk may have increased cash due to precautionary savings motives (Opler, Pinkowitz, Stulz, and Williamson, 1999; Bates, Kahle, and Stulz, 2009). Or financially constrained firms may have increased cash (Almeida, Campello, and Weisbach, 2004; Han and Qiu, 2007) or lowered debt outstanding (Acharya, Almeida, and Campello, 2007) to finance future investment opportunities. Alternatively, financially constrained firms may have used the tax refunds to increase investment. A large literature dating back to Fazzari, Hubbard, and Petersen (1988) shows that cash flow can be important for investment decisions in financially constrained

³ A statement from Treasury Secretary Paul O'Neill after passage of the 2002 policy read: "This legislation will add momentum so that we have a more robust economic recovery and return to full prosperity.... [T]his legislation....will speed America back to work" (U.S. Treasury, 2002). A statement from the House Ways and Means Committee describing the 2009 policy prior to passage stated that the legislation would give firms "cash infusions that would help them weather the current economic storm" (Ways and Means Committee, 2009). And a post-passage White House fact sheet stated that the legislation "creates jobs by cutting taxes for struggling businesses....putting \$33 billion of tax cuts in the hands of businesses this year when they need it most" (White House, 2009).

⁴ Seminal models include Modigliani and Miller (1958), Tobin (1969), Abel (1983), and Hayashi (1982).

firms.⁵ Finally, the choice of whether to use the tax refund for investment or another use may also have depended on overall economic conditions, as Bolton, Chen and Wang (2011 and 2013). Bolton, Chen, and Wang (2013), for example, show that firms are more likely to hoard cash and reduce investment in a crisis state as compared to a non-crisis state.

To study the effects of the five-year carryback policy, I first examine how firms allocated the tax refunds. Did firms increase investment or did they choose another use of funds such as increasing cash holdings, reducing debt, or making payouts to shareholders? Next, I analyze if the policy improved firm financial positions overall, testing whether receiving a tax refund affected firm bankruptcy risk, credit ratings, and the probability of bankruptcy or liquidation.

I document two main findings. First, I show that firms chose different uses for the tax refunds in the two policy periods. After passage of the 2002 policy, I find that firms allocated the funds to investment (\$0.40 of every refund dollar) in the year they received the refund and I find no effect in the following years. In contrast, I find that after passage of the 2009 policy, firms first held the tax refunds as cash and then used the refunds to pay down long-term debt. In 2010—the year of refund receipt—firms allocated \$0.96 of every refund dollar to higher cash holdings. Then in 2011, firms reduced cash holdings and reduced long-term debt outstanding (\$1.26 out of every refund dollar). I find no effects on the use of funds in later years and no effect on investment in any year of the 2009 policy period. I also find no effects of the policy on the change in employment—a key policy goal—in either the 2002 period or the 2009 period.

The difference in responses between policy periods is consistent with a hypothesis that firms may choose different uses of liquidity under different economic conditions, as in Bolton, Chen and Wang's (2011, 2013) work. Macroeconomic conditions across the two policy periods were very different. The 2001 recession was the mildest in post-war history—i.e., one of the shortest recessions with the smallest real GDP declines—though credit conditions for firms were tight

⁵ Some of the notable papers in corporate finance regarding financial constraints include, for example, Blanchard, Lopez-de-Silanes, and Shleifer (1994), Lamont (1997), Rauh (2006), and Hennessey and Whited (2007). Even when firms are unconstrained, Gomes (2001) shows that cash flow can be an important determinant of investment when external finance is costly.

during and following the recession. The 2007-2009 recession, on the other hand, was the most severe since the Great Depression, and was characterized by high volatility in markets, weak expectations for future economic conditions, credit-market freezes, and tight credit conditions overall. (In Section 1.3, I provide additional background on economic conditions during the two recessions.)

The use of tax refunds to increase investment in the 2002 period suggests that firms in my sample were financially constrained, but had profitable investment opportunities at the time. In line with this hypothesis, I find that the increase in investment was concentrated in financially constrained firms and in firms with the highest investment opportunities. The finding that in the 2009 policy period, firms used the tax refunds to increase cash holdings is consistent with the hypothesis that firms built cash either due to a precautionary savings motive in the face of high uncertainty, or for financially constrained firms to fund future investment opportunities. My results suggest the uncertainty channel was the larger motivation for firms to increase cash. I find the cash increase was concentrated among firms with high historical cash-flow volatility and realized stock-price volatility. I do not see the cash increase concentrated in financially constrained firms.

The second main finding of this paper is that although the policy had no discernable effect on investment in the 2009 period, it did improve financial conditions overall in that period. I find that the tax refunds lowered the probability of a future credit rating downgrade and lowered firms' bankruptcy risk on average (as measured by distance to default, Altman's z-score, and Ohlson's o-score). These findings are consistent with my results that firms first held the tax refunds as cash in 2010 and then reduced cash in order to pay down long-term debt in 2011. These decisions would tend to reduce firm riskiness overall. The tax refund policy had a weaker effect on measures of financial conditions in the 2002 policy period. I only see a small effect on Altman's z-score. The policy had no effect staving off severe negative outcomes in either period, however. I find no effect on actual bankruptcy events.

I demonstrate that these results are generally robust to the empirical specification—to implementing a “sharp” RKD strategy instead of the preferred “fuzzy” RKD strategy, to the polynomial form of the regression specification, to narrowing the regression window around the kink point, and to excluding industries that were particularly hard hit in each recession (communications and airlines in 2002 and homebuilders in 2009). I find reasonable estimates of the average firm tax rate in both policy periods: 34 percent in the 2002 period and 31 percent in the 2009 period. These estimates are not statistically different from each other and are close to the top marginal corporate tax rate of 35 percent. Finally, I find no evidence of confounding interactions with other firm-related fiscal policy measures enacted at the same time, such as “bonus” depreciation of investment expenses.

This work relates to two major literatures in finance and economics. First, it relates to the literature that provides empirical evidence on the effects of fiscal policy targeted at firms.⁶ This paper is the first to study the economic effects of the tax carryback policy.⁷ In contrast to previous literature, I directly consider potential differences in responses across policy periods and show that firm responses can differ depending on economic conditions. I also study the effects of fiscal stimulus policies on firm financial conditions more broadly (i.e., bankruptcy risk and credit quality), in addition to studying how firms used fiscal stimulus funds.

⁶ A larger body of literature studies the effects of fiscal policy measures directed at consumers or direct government spending initiatives. For example, Johnson, Parker and Souleles (2006) and Agarwal, Liu and Souleles (2007) show that the 2001 income tax rebates caused a substantial increase in consumer spending after disbursement, particularly for liquidity constrained households. Johnson, McClelland, Parker and Souleles (2011) find a similar result for the 2008 economic stimulus payments. Wilson (2010) estimates the effect of spending from the American Recovery and Reinvestment Act of 2009 (ARRA) broadly and finds that ARRA spending added 0.8 job-years per every \$100,000 of spending. Chodorow-Reich et al. (2011) show the ARRA provisions that increased federal Medicaid aid to states resulted in an additional 3.8 job-years for every \$100,000 in Medicaid spending. Mian and Sufi (2012) study the 2009 “Cash for Clunkers” policy and find that the policy had a large short-term effect in boosting automobile purchases but that most of the effect was reversed within the next 10 months.

⁷ Boyton and Cooper (2003) and Graham and Kim (2009) estimate the total value of the carrybacks and tax refunds to firms for varying carryback windows, but do not study the effects of the policy as a stimulus measure. Graham and Kim (2009) also estimate how the carryback policy would affect firm marginal tax rates in the 2009 period and hence, firm debt ratios, using estimates of the relationship between marginal tax rates and debt from Graham (1996). They estimate that the Obama Administration’s proposed policy would provide substantial additional liquidity to firms—\$19 billion and \$34 billion in 2008 and 2009, respectively, which would increase firm debt capacity by \$8 and \$10 billion in those years. Cohn (2011) studies the effect of net operating loss carryforwards on firm investment and shows that higher taxes result in firms reducing investment.

Analyses of other stimulus policies targeted at firms have found mixed real effects to date. The most widely studied firm fiscal stimulus policy was the 2004 tax holiday on the repatriation of foreign earnings. Blouin and Krull (2009) and Dharmapala, Foley, and Forbes (2011) find that the main effect of the holiday was to increase shareholder payouts. This result suggests U.S. multinational firms were not financially constrained during the policy period. In contrast to the repatriation holiday, the tax carryback policy explicitly targeted firms with losses during the recession and not just large, multinational firms. Firms with losses may have been more likely to be financially constrained and, as I find in the 2002 period, more likely to use the funds for investment. In this vein, Faulkender and Petersen's (2012) study of the repatriation holiday shows that highly financially constrained firms used the repatriated earnings for investment. Studies of "bonus" depreciation—a policy that accelerated the schedule for deducting investment from taxable income—also find a substantial effect of the policy on firm investment (House and Shapiro, 2008; Mahon and Zwick, 2014). Mahon and Zwick's results suggest that bonus depreciation's effect on investment stemmed from a cash-flow channel—by lowering tax liabilities for financially constrained firms. They show that the policy's effect on investment is concentrated among financially constrained firms and profitable firms, not in unconstrained firms or in firms with losses (hence zero tax liability). In contrast, my sample covers firms with losses and I show the tax carryback policy increased investment for these firms in the 2002 period.

Second, this paper relates to the literature that studies the role of financial constraints and uncertainty in propagating business cycles. A large body of work in macroeconomics shows that financing frictions can cause and amplify business cycles (Kiyotaki and Moore, 1997; Bernanke, Gertler, and Gilchrist, 1999).⁸ While I am not directly testing implications of these models, my paper provides empirical evidence on the role that external financial constraints played in the recoveries from the last two U.S. recessions. My result from the 2002 policy period—that financially constrained firms used the tax refunds to increase investment—suggests financing constraints did indeed play a role restraining investment following the 2001 recession. In the 2009

⁸ Brunnermeier, Eisenbach, and Sannikov (2012) survey the literature on financing frictions in macroeconomics.

period, in contrast, I find that when the NOL carryback extension lifted firm financial constraints by providing additional liquidity, firms did *not* invest the additional funds. Instead, firms held the funds as cash, which suggests that financing constraints may not have been the key friction restraining investment after the 2007-2009 recession. My results suggest that instead, high uncertainty may have been playing a larger role in restraining investment. In particular, I find that the cash holdings result was concentrated in firms facing higher uncertainty about future prospects. A rapidly growing literature stemming from work by Bloom (2009) highlights the role of aggregate uncertainty shocks as another important channel that causes and propagates business cycles. This work is grounded in the real options theory literature, which shows that firms delay investing until economic uncertainty is resolved over time or until the benefits of investment become sufficiently large (Cukierman, 1980; Bernanke, 1983; Pindyck, 1991).

The remainder of the paper proceeds as follows. Section 1.2 provides background on the applicable U.S. tax code statutes and the NOL carryback policy implementation. Section 1.3 gives some background on the economic conditions in each policy period and Section 1.4 describes the empirical strategy and the data sample. I present my results on the effects of the policy in Section 1.5 and conclude in Section 1.6.

1.2 Policy Background

1.2.1 U.S. Tax Code

In any given year, a firm sustains a net operating loss (NOL) for tax purposes when its allowable tax deductions exceed gross income. Under section 172 of the Internal Revenue Code, these losses can be used in two ways. First, they can be used to offset taxable income in either of the prior two years, for which the firm receives a tax refund. This policy is known as an “NOL carryback.” Alternatively, if the firm does not have positive taxable income in the prior two years or elects not to use its carryback, it can carry the loss forward for up to twenty years and use it to offset future taxable income, thereby lowering its tax bill at some point in the future. This is known

as an “NOL carryforward.”⁹ According to the Joint Committee on Taxation (JCT), “the intent of the NOL carryback/carryforward provision is to give taxpayers the ability to smooth out changes in business income, and therefore taxes, over the business cycle.”¹⁰ In 2002 and then again in 2009, Congress enacted legislation extending the NOL carryback window from two years to five years. I describe each policy action in Section 1.2.2.

I present an example of how the carryback deduction is applied under the two-year policy and the five-year extension policy in Table 1. The example firm had a \$100 million NOL in 2001 (hence, a maximum potential NOL deduction of \$100 million) and profits ranging from \$50 million in 1996 to \$10 million in 2000 before taking the NOL deduction into account. I assume a tax rate of 35 percent in each year.

Under the two-year carryback policy, the firm could take a \$20 million NOL deduction in 1999 and a \$10 million deduction in 2000—receiving a tax refund of \$10.5 million ($0.35 \times \30 million). In this case, the firm keeps a \$70 million NOL to carry forward in the future (\$100 million minus \$20 million minus \$10 million). Under the five-year carryback extension policy, the firm’s tax refund was substantially larger. This firm could take a \$50 million carryback deduction in 1996, a \$40 million deduction in 1997 and a \$10 million deduction in 1998. Since the firm can now deduct the full \$100 million NOL, it receives the maximum tax refund of \$35 million and has no losses remaining to carry forward. The firm receives an additional \$24.5 million as a result of the carryback extension. (This calculation assumes the firm would take the deductions starting in the earliest year of the window.)

⁹ Using an NOL carryforward is fairly straightforward: Firms enter the amount of deduction they would like to take on line 29 of form 1120 when they file their tax returns. In the case of a carryback, they have the option to file form 1139 in the 12 months following the end of the taxable year in which the loss is incurred. After that one year period, they can still use the carryback and get a refund by filing an amended tax return using Form 1120X. Firms keep track of their NOL carryovers and report the total on Schedule K of IRS Form 1120. Also on Schedule K firms incurring a loss can make an election to permanently forego the carryback of that loss. In mergers and acquisitions, there are special rules that limit an acquiring company’s use of NOLs on the books of a target firm. There are also special rules governing the use of NOLs to offset Alternative Minimum Taxable Income, governing the use of NOLs for life insurance companies, and governing farming losses, disaster losses, casualty loss, etc...

¹⁰ U.S. Congress, Joint Committee on Taxation, Estimated Budget Effects of The Chairman’s Amendment in The Nature of a Substitute to H.R. 598, The “American Recovery and Reinvestment Tax Act of 2009”, 111th Cong., 1st sess., January 22, 2009, JCX-9-09.

1.2.2 The Job Creation and Worker Assistance Act of 2002 (JCWA)¹¹

The JCWA was introduced in October 2001 and signed into law in early March 2002, allowing firms to carryback losses incurred in tax years 2001 and 2002 for five years instead of the usual two. Losses in 2001 could be carried back to offset income in 1996, 1997, and 1998, in addition to income in 1999 and 2000. At the time of passage, the JCT estimated that the NOL provision would return an additional \$7.9 billion in tax refunds to firms in 2002 (for losses incurred in tax year 2001), and \$6.6 billion in tax refunds to firms in 2003 (for tax losses incurred in 2002). Over a 10-year horizon, the JCT estimated that the NOL provision would have a revenue cost of about \$2 billion, reflecting the future reduction in carryforwards available to offset taxable income. The JCWA also included measures to extend a number of expiring tax code provisions, to provide an extra 30 percent first-year expensing for qualified capital investments (known as bonus depreciation), and to extend unemployment insurance benefits for workers.

1.2.3 The American Recovery and Reinvestment Act of 2009 (ARRA)

The 2009 policy was enacted in two separate pieces of legislation. As part of the ARRA—the \$830 billion stimulus package that was signed into law in February 2009—Congress extended the carryback window for losses incurred in tax year 2008. This policy was limited to small businesses, i.e., those with less than an average of \$15 million in gross receipts per year over the previous three years. The JCT estimated that the policy would return \$4.7 billion in refunds to firms in 2009 with a 10-year cost of about \$1 billion. The five-year carryback was small relative to the overall package, which also included an extension of the bonus depreciation allowance (an extra 50 percent of first-year expensing), a payroll tax credit, an additional child tax credit, and additional government funding for health care, education, and infrastructure.

¹¹ For cost estimates of each of the three pieces of legislation, see the following Joint Committee on Taxation publications: Estimated Revenue Effects of the “Job Creation and Worker Assistance Act of 2002”, March 6, 2002, JCX-13-02; Estimated Budget Effects of the Revenue Provisions Contained in the Conference Agreement for H.R. 1, the “American Recovery and Reinvestment Act of 2009”, February 12, 2009, JCX-19-09; and Estimated Revenue Effects of Certain Revenue Provisions Contained in the “Worker, Homeownership, and Business Assistance Act of 2009”, November 3, 2009, JCX-45-09.

1.2.4 The Worker, Homeowner, and Business Assistance Act of 2009 (WHBA)

The Administration budget released in May 2009 included a proposal to allow the carryback to apply to all firms and to apply to losses in both 2008 and 2009. Congress introduced legislation to this effect in September 2009 and passed the WHBA to extend the five-year carryback window in November 2009. The carryback extension could only be applied to either 2008 losses or 2009 losses, not both. The exception was for firms that qualified for the policy under the ARRA. These firms were allowed to apply the extension to both years. Firms were only allowed to apply 50 percent of taxable profits in the earliest year of the extension window to the policy. Also, firms that received assistance under the Troubled Asset Relief Program (TARP) were excluded from participating. The JCT estimated that the policy would return an additional \$33 billion to taxpayers in 2010 and that the expected 10-year cost of the program would be \$10.4 billion.

1.3 Empirical Strategy and Data Description

1.3.1 Regression Kink Design Overview

To estimate the effects of providing tax refunds under the five-year carryback policy, I use a regression kink design (RKD) strategy. This strategy takes advantage of a discontinuity in the slope of the formula that determines the size of a firm's tax refund. In general terms, RKD identifies the causal effect of a particular policy variable on an outcome variable by using "kinks"—discontinuities in slope—in the assignment rule for the policy variable (Card, Lee, Pei, and Weber, 2012). For example, one can test the effect of unemployment insurance benefits (the policy variable) on the duration of joblessness (the outcome variable) based on the phase-out of unemployment insurance benefits at higher income levels (the assignment variable) (Card, Lee, Pei, and Weber, 2012). Another example would be to test the effect of the earned income tax credit (the policy variable) on labor force participation (the outcome variable) based on the phase-out of the EITC at higher income levels (the assignment variable) (Jones, 2011).

The intuition behind the strategy is that the causal effect of a policy can be estimated by testing for a kink in the outcome variable that occurs at the kink in the assignment variable. RKD is a similar concept to regression discontinuity design (RDD). RDD identifies an effect using a discontinuity in the *level* of the function that relates an assignment variable to the outcome variable. RKD identifies an effect using a discontinuity in *slope* of the function.

As with RDD, RKD has a “sharp” and “fuzzy” variant. In sharp RKD, the change in slope that occurs at the kink point is precisely known and is equal for all affected entities. Fuzzy RKD, on the other hand, uses an estimate of the average change in slope across agents based on the observed data. In my setting, the estimated slope in the tax refund corresponds to the average tax rate of firms, as discussed below. I implement a fuzzy RKD strategy, therefore, to account for differences in average tax rates between firms and because I am only able to estimate firms' tax refunds based on taxes paid as reported in Compustat. In robustness tests, I present estimates from a sharp RKD strategy as well.¹²

1.3.2 Regression Kink Design Applied to the NOL Carryback Policy

In my empirical setting, the outcome variables of interest are firm uses of cash flow (e.g., investment, change in cash holdings, and change in debt) and measures of financial health (e.g., bankruptcy risk and credit ratings). The policy variable is the size of a firm's tax refund. The assignment variable that determines the value of a firm's tax refund is a function of positive taxable income (which I will call profits) in previous years and the size of losses in a given policy year. Under the five-year carryback policy, a firm that incurs a loss in tax year 2001, for example, can receive a refund for taxes paid from 1996 to 2000 until the point where total profits from those years equals the 2001 loss. This statutory condition introduces a kink in cash available to a firm from the tax refund at the point where losses in 2001 equal previous-years' profits. (I detail the

¹² The marginal corporate tax rate is at least 34 percent for any firm with taxable income greater than \$75,000 and is 35 percent for taxable income greater than \$18 million. The average corporate tax rate (on positive taxable income) may be expected to be close to 35 percent for most Compustat firms, therefore. My findings are similar assuming a constant tax rate in a sharp RKD strategy (Table A1).

construction of firms' tax refunds and taxable income that can be applied to the policy in Appendix 1.)

As an example, take three firms that each sustained a \$100 million loss in 2001. Firm A earned \$80 million in profits from 1996-2000, firm B earned \$100 million in profits, and firm C earned \$120 million in profits. Under the five-year carryback policy, firm A would receive a \$28 million tax refund ($\80×0.35, assuming a 35 percent tax rate), while both firms B and C would receive a \$35 million tax refund. Although firm C had a higher level of profits from 1996-2000, firm C can only receive a refund for taxes paid in previous years until the point where previous profits equal current losses.

Figure 1 shows an example of the kink in the tax refund formula for a set of firms with \$100 million in 2001 losses and varying amounts of profits from 1996 to 2000 (assuming a 35 percent tax rate). The firm's tax refund is a function of two variables: 1) profits over the five-year carryback window, and 2) the firm's policy-year losses. The kink point occurs where previous-years' profits equal policy-year losses: at \$100 million in this example. To the left of the kink, in the region where past profits were less than current losses, the slope of the tax refund function is the firm's tax rate. For every extra dollar of past profits, a firm receives an extra \$0.35 in tax refund. To the right of the kink—the region where past profits exceed current losses—the slope of the function is zero. A firm receives no additional refund for an additional dollar of past profits in this region.

This example illustrates the tax refund function for firms with \$100 million in losses. In my sample, however, I have firms with a wide range of losses in the policy years. Each firm would receive its maximum refund at the point where their policy-year losses equal their previous-years' profits. To standardize the tax refund function across firms, therefore, I generate one assignment variable to describe the function: previous-years' profits *minus* policy-year losses. The kink point in this variable occurs at zero for all firms. As in Figure 1, when previous-years' profits are less than

policy-year losses, the slope of the function is the firm's tax rate. When previous-years' profits are greater than policy-year losses, the slope is zero.

1.3.3 A General RKD Model

In this section, I describe the RKD methodology in detail. As in Nielsen, Sorensen and Taber (2010), let the following model represent the general, causal relationship between an outcome variable of interest (Y) and a policy variable of interest (X):

$$Y = \beta_1 X(V) + g(V) + \epsilon,$$

In this model, X is a deterministic and continuous function of the assignment variable V and the function relating X and V has a kink at $V = V^*$. The outcome variable Y may be a direct function of V as well— $g(V)$ —and the error term ϵ is a vector of unobservable random variables. In my setting, Y is the firm outcome variable, X is the tax refund, and V is total profits over the five-year carryback window minus policy-year losses. As discussed above, the kink occurs where previous-years' profits equal policy-year losses ($V^*=0$).

The typical problem in evaluating a model like the one above is that the error term ϵ is correlated with X, leading to bias in estimates of β_1 . In RKD, however, if $g(V)$ and $E(\epsilon|X)$ have no kink in V at V^* —i.e., they have derivatives that are continuous in V at $V = V^*$ —then the parameter of interest β_1 identifies the causal effect of X on Y and is equal to the following term:

$$\beta_1 = \frac{\lim_{v \uparrow v^*} \frac{dE[Y|V=v]}{dv} - \lim_{v \downarrow v^*} \frac{dE[Y|V=v]}{dv}}{\lim_{v \uparrow v^*} \frac{dX(v)}{dv} - \lim_{v \downarrow v^*} \frac{dX(v)}{dv}} \quad (A)$$

The expression on the right hand side of the equation is the change in the slope of the conditional expectation of Y given the assignment variable V at the kink point, divided by the change in the slope of the deterministic function that relates X and V at the kink point. The policy's effect is identified by estimating the kink in the outcome variable with respect to the assignment variable

and then making an adjustment for the magnitude of the kink in the policy variable with respect to the assignment variable.

Card, Lee, Pei, and Weber (2012) show that under two major identifying assumptions, expression (A) can recover the treatment on the treated parameter in a generalized non-separable model as well:

$$Y = f(X, V, W),$$

where Y is the outcome variable, X is the policy variable of interest, V is the assignment variable that enters the model with a “kink” at V^* , and W is an unobservable, non-additive error term.

The first identification assumption in Card et al. (2012) is that the probability density function of firms is continuously differentiable in V —i.e., the density is smooth around the kink point. In short, all firms cannot be able to perfectly choose the level of current losses relative to past profits that they can apply to the tax refund policy. It is worth noting that this identification assumption does not require that firms could in no way manipulate their tax refund position. Indeed, firms have incentives to do extensive tax planning (Armstrong, Blouin, and Larker, 2012). Instead, the identification assumption requires that there is sufficient randomness such that firms cannot perfectly sort themselves on either side of the kink point. During recessionary periods, firms face unanticipated negative shocks; it may be expected that they would have less room to maneuver their tax position in these periods.

The second assumption is that pre-determined firm characteristics are continuously differentiable with respect to V around the kink point. In other words, firms must be similar in other respects around the kink point so as to be comparable. If firms have a kink in characteristics on either side of the kink point, these other characteristics may be driving the result, rather than the policy variable of interest driving the result. I provide evidence that suggests both assumptions are satisfied in my setting in Section 1.3.6.

1.3.4 RKD Empirical Specification

To estimate a fuzzy RKD, I use a two-stage least squares instrumental variable (IV) strategy (Card, Lee, Pei, and Weber, 2012). In the first stage, I estimate the change in slope of the policy variable—the tax refund—at the kink point where previous-years' profits equal policy-year losses. The variable that estimates this change in slope is an excluded instrument in the IV strategy. In the second stage, I use the fitted values of the tax refund to estimate the effect of the tax refund on firm outcomes. I describe each stage below:

First-Stage Regression:

The empirical specification for the first-stage regression is as follows:

$$(1) \text{TaxRefund}_{it} = \delta_0 + \sum_{p=1}^P [\delta_p (V_{it-1} - V^*)^p + \gamma_p D \cdot (V_{it-1} - V^*)^p] + \theta \text{Controls}_{it} + \omega_n + \epsilon_{it},$$

where i is firm, t is year, and n is industry. V is the assignment variable (previous-years' profits minus policy-year losses) and V^* is zero. I use the level of V (in millions of dollars) in the regression as opposed to scaling V by assets or another measure because the policy kink occurs in the level of the tax refund and V . In the context of Figure 1, the function relating V and the tax refund is linear in the level of V to the left of the kink point. This function is not linear in V as a share of assets or in a re-centered logarithm of V .

The instruments for Tax Refund are a dummy variable that equals one if previous-years' profits were less than policy losses (D) interacted with a polynomial in V . In the context of Figure 1 above, D is a dummy variable that equals one if a firm is to the left of the kink point. The coefficient γ_1 in this specification recovers the change in the slope of the tax refund value with respect to V around the kink point (the denominator from the estimand in expression A). In my setting, this value is equal to the average estimated tax rate.

Under the identification assumptions of regression kink design, the instruments satisfy the assumptions required for a valid IV strategy. In my setting, the instruments satisfy the relevance

condition because location relative to the kink point strongly affects the size of a tax refund, as illustrated above. The exclusion condition—that being on one side of the kink point or the other does not affect firm outcomes through another channel besides the size of the tax refund—also appears reasonable. The kink point is a statutory requirement and one of the model identification assumptions is that pre-determined firm characteristics have a smooth distribution around the kink point. (This assumption appears to be satisfied in my setting as I show below.) Because firm characteristics are similar around the kink and evolve smoothly, but the kink point is a sharp discontinuity set in law, it is reasonable that being above or below the kink has no effect on firm outcomes except through the formula that determines the tax refund.

My preferred polynomial order is $P = 2$, in line with other RKD studies, and I present robustness to other polynomial orders in Table A2. I include the following controls in the regression: pre-treatment values of Tobin's q , return on assets, cash flow/assets, sales/assets, leverage, the firm's marginal tax rate, the log of assets and a quadratic in the value of losses that can be applied to the policy. I include Fama-French 48 industry fixed effects to account for macroeconomic shocks that affect industries differently and cluster the standard errors at the Fama-French 48 industry level to account for unobserved correlation in errors within industries.

Second-Stage Regression:

The empirical specification for the second-stage regression is as follows:

$$(2) \text{ FirmOutcome}_{it} = \alpha_0 + \beta_1 \widehat{\text{TaxRefund}}_{it} + \sum_{p=1}^P [\alpha_p (V_{it-1} - V^*)^p] + \theta \text{Controls}_{it} + \omega_n + \epsilon_{it},$$

where i is firm, t is year, n is industry. $\widehat{\text{TaxRefund}}$ is the fitted values from the first-stage regression and V is the assignment variable as described above. I include the same controls as in (1) and I cluster the standard errors at the Fama-French 48 industry level.

I study two types of outcome variables of interest: potential uses of the tax refunds (i.e., investment, payout and the change in cash holdings) and measures of firm financial conditions

including bankruptcy risk and credit conditions (Altman's z-score, Ohlson's o-score, distance-to-default, S&P credit rating upgrades and downgrades, and actual bankruptcy or liquidation). For analyzing the potential uses of funds variables, I use the level of spending, in millions of dollars. I use levels because the kink in the tax refund formula occurs in the level of V , and I would expect the corresponding kink to occur in the level of the potential uses of funds. Measuring these variables in levels also results in a convenient interpretation of β_1 as a firm's marginal propensity to invest or otherwise allocate the funds out of every additional dollar of the tax refund.

Finally, I study the effects of the 2002 policy and the 2009 policy separately. The time dimension of the above regressions varies depending on the policy period. For the 2002 policy, firms received refunds in 2002 and in 2003 for losses incurred in 2001 and 2002, respectively. I regress firm outcomes in 2002 and 2003 as a function of the tax refunds received in those years and the assignment variable V in the previous year. This is a two-year panel regression. For the 2009 policy, most firms received only one refund—in 2010—for losses in 2008 or 2009. For the empirical specification of the 2009 policy period, I regress firm outcomes in 2010 (or 2011) on the value of the tax refund received in 2010 and the assignment variable V at the end of 2009. This is a one-year, cross-sectional regression.

1.3.5 Data Description

In this analysis, I use financial variables from Compustat and CRSP as well as data on S&P credit ratings from Capital IQ, analyst forecast dispersion from I/B/E/S, and marginal tax rates provided by John Graham (Graham, 1996).

The crux of the analysis relies on calculating the firm tax refund and generating the assignment variable V —profits (positive taxable income) available to apply to the policy from the five-year carryback window minus total losses (negative taxable income) to apply to the policy. As taxable income is not available on firm financial statements, I calculate an estimate of taxable income based on Compustat data in a manner similar to Graham and Kim (2009). The difference

between my measure of taxable income and the Graham and Kim measure is that I calculate a U.S.-specific measure because a firm's tax refund under the policy is based only on U.S. taxable income and taxes paid. The Graham and Kim (2009) measure is based on worldwide pre-tax income. I then calculate total profits and total losses that can be applied to a carryback for a given policy year, as well as the firm's tax refund following Graham and Kim (2009) and Boynton and Cooper (2003). These calculations are detailed in Appendix 1.

Following Graham and Kim (2009), I require that firms in my data sample: 1) experience a loss that can be applied to the policy, 2) are present in Compustat for the five-year window required to calculate the carryback value, 3) and have total assets of greater than \$1 million. To remove the influence of extreme outliers, I exclude firms in the 1 percent tails of V , the 1 percent tails of investment (for variables measuring potential uses of the tax refund), and a few extreme outlier points.¹³

Table 1 shows summary statistics for the outcome and control variables for all firm-years in the sample. Appendix 2 presents details on the construction of each variable.

1.3.6 Empirical Strategy Validity

I present evidence that the RKD identification assumptions hold in Figure 2, Figure 3, and Table 1. Regarding the first assumption, Figure 2 shows a histogram of firms around the $V = 0$ kink point in \$0.25 million bins of the assignment variable V for both the 2002 policy period and the 2009 policy period. Though the distributions are somewhat noisy—particularly in the 2009 period—there is no obvious discontinuity around the kink point.¹⁴

¹³ For example, in the 2002 policy period sample, I observe one firm (Lucent Technologies) with an estimated tax refund of \$2.5 billion, whereas the second largest estimated tax refund is \$1.1 billion. I calculate an average federal tax rate of 50.5 percent for Lucent Technologies from 1996-2000 and in 2000, the estimated rate is particularly unrealistic (92 percent), suggesting my methodology has overestimated this refund. I exclude this firm from the sample, therefore.

¹⁴ As further evidence that firms could not perfectly manipulate their value of V , Figure A3 in Appendix 3 shows the histogram of V around the kink point separately for firms as of 2001 and as of 2002. The policy applied to losses for 2001 and 2002, so firms received separate tax refunds in 2002 and in 2003. As the carryback policy was passed into law in March 2002, the refund based on 2002 losses would have been anticipated early in the year but the refund based on 2001 losses would have been more uncertain. If firms were able to perfectly manipulate their levels of V , I would expect to see a discontinuity in the distribution V for the 2002 policy period, but that does not appear to be the case.

Regarding the second assumption—that pre-determined firm characteristics have a smooth distribution around the kink point—Figure 3 shows the average value of a number of pre-treatment firm characteristics for firms in \$1 million bins above and below the zero kink point for the 2009 policy data set: leverage, Tobin's q, return on assets, and the book value of total assets. While again the distributions of these variables are noisy, they appear to evolve fairly smoothly around the kink point. I test for a kink in characteristics more formally by collapsing the data into \$0.5 million bins and estimating the following specification in a narrow window around the kink point (-\$25 million to \$25 million) for both the 2002 and 2009 policy periods as in Turner (2014):

$$(3) Y_{it-1} = \alpha_0 + \alpha_1(V_{it-1} - V^*) + \beta_1 D \cdot (V_{it-1} - V^*) + \epsilon_{it}$$

In this specification, Y is the re-centered residual from a regression of a firm characteristic on industry fixed effects (to control for industry-specific effects). The coefficient β_1 recovers the change in slope around the kink point. A statistically significant coefficient would suggest a kink in the characteristic, but I find no evidence of this in either policy period (Table 3).

Another issue of concern for the validity of my results is any potential confounding interaction of other firm-related fiscal policy measures that were enacted around the NOL carryback extension. One major policy enacted in both 2002 and 2009 was “bonus” depreciation, which accelerated the schedule for deducting investment expenses from taxable income. Because investment is deductible from taxable income, this policy would be a concern for my identification strategy if it caused certain types of firms to accelerate investment and increase losses such that firm characteristics differed from one side of the kink point to the other.

Two factors suggest that bonus depreciation is not confounding my results. First, Mahon and Zwick (2014) find the policy only raised investment for profitable firms, and had no effects on investment for firms with losses. Since my sample is restricted to firms with losses, this result suggests that the bonus depreciation policy is not confounding my results. Second, I test if firms that are more likely to take advantage of bonus depreciation sort deterministically to one side of

the kink point or the other. Mahon and Zwick (2014) show that bonus depreciation would be more beneficial to firms in industries that invest in long-lived assets than invest in short-lived assets. If bonus depreciation was causing predominately long-duration asset firms to increase investment (and hence, increase their losses) and deterministically sort to one side of the kink point, I would expect to see evidence of such sorting in the distribution of these firms. Appendix 3, Figure A3 shows histograms of V for firms with the highest value of the depreciation allowance (the long-duration asset firms) compared to firms with the lowest value of the deprecation allowance (the short-duration asset firms).¹⁵ The distribution of firms looks relatively smooth around the kink point in both cases.

Other potential confounding tax policy measures are the dividend tax cut of 2003, the tax repatriation holiday on foreign earnings passed in 2004, and the extension of expiring tax provisions passed in 2002. However, none of these policies would have been expected to cause a discontinuity in firm characteristics around the kink point in the NOL tax refund function. The kink point remains a statutory requirement that is unrelated to these other policies. The dividend tax cut passed in 2003 reduced the top tax rate on U.S. dividend income from 38.6 percent to 15 percent. Yagan (2013) studies the effect of this tax cut on firm investment, and finds no differential effects by firm size or other firm characteristics. The repatriation holiday affected repatriations in 2005—outside of my analysis window. Finally, the 2002 NOL policy was passed concurrently with extensions of expiring tax provisions including the research and experimentation (R&E) tax credit, a credit for the production of electricity from wind, and the work opportunity tax credit. These provisions are widely expected to be extended each year. For example, the R&E tax credit was originally passed in 1981 and has been extended 15 times.

¹⁵ Following Mahon and Zwick (2014), I separate firms into the ten most common three-digit NAICS industries in the top three deciles of the present discounted value of a dollar of deductions for investment—firms that benefit the most from bonus depreciation—and the ten most common industries in the bottom three deciles. In the top three deciles are: professional, scientific and technical services (541), specialty trade contractors (238), computer and electronic product manufacturing (334), durable goods wholesalers (423), construction of buildings (236), heavy and civil engineering construction and land subdivision (237), truck transportation (484), rental and leasing services (532), nondepository credit intermediation (522), and administrative and support service (561). In the bottom three deciles are: motor vehicle and parts dealers (441), food manufacturing (311), real estate (531), telecommunications (517), fabricated metal product manufacturing (332), food services and drinking places (722), transportation equipment manufacturing (336), oil and gas extraction (211), nondurable goods wholesalers (424), and primary metal manufacturing (331).

1.4 Background on Economic Conditions: 2002 vs. 2009 Policy Period

While the tax refund policies enacted in 2002 and 2009 were similar, economic conditions across the two recessions and recovery periods were quite different. To give context to the policy analysis, I offer some brief background on the two periods in Figure 4. There are three main takeaways. First, economic conditions and perceptions about future economic conditions were much weaker in the 2007-2009 recession than in the 2001 recession. Real GDP declines during the 2007-2009 recession were the largest since the Great Depression. In contrast, there were only two mild quarterly declines in the 2001 recession (Figure 4.A). Furthermore, CFO optimism about future economic performance was lower during the 2009 post-recession period in than the 2001 post-recession period, as were professional forecaster's expectations about future GDP growth (Figure 4.B).

The second takeaway is that credit conditions were much worse during the 2007-2009 recession than the 2001 recession, but conditions were tight in both recovery periods. Figure 4.C shows that the Baa-Aaa corporate bond spread and the TED spread (the 3-month LIBOR minus the 3-month Treasury bill rate—a measure of interbank lending conditions) spiked in 2008 and 2009. The Baa-Aaa spread remained elevated in both recoveries, however, and the net percentage of domestic banks reporting tighter lending standards for commercial and industrial (C&I) loans was elevated longer in the 2001 recovery period than the 2007-2009 recovery period (Figure 4.D).

The third takeaway is that measures of uncertainty about future economic conditions were higher in the 2007-2009 recession than the 2001 recession and economic policy uncertainty was higher in the 2007-2009 recovery period. Stock volatility as measured by the VIX hit record levels in the 2007-2009 recession (Figure 4.E), though after the recession, the VIX fell to levels that followed the 2001 recession. Dispersion in professional forecaster's future expectations of growth was also particularly high in the 2007-2009 recession, though it fell following the recession. Economic policy uncertainty, on the other hand, as measured by the Baker, Bloom, and Davis index,

remained substantially elevated in the 2007-2009 recovery period as compared to the 2001 recovery period (Figure 4.F).

1.5 Firm Responses to the NOL Carryback Extension Policy

1.5.1 First-Stage Estimates of the Tax Refund Rate

I begin by presenting results from the first-stage regression (specification 1) that estimates the change in slope of the tax refund function around the kink point. As discussed above, this change should recover the average firm tax rate in the two carryback window periods. Table 4 shows the average tax rate estimates for both the 2002 policy and the 2009 policy: 34 percent and 31 percent, respectively. These coefficient estimates are not statistically different and are in a reasonable range for average statutory corporate income tax rate estimates.¹⁶ Observing a reasonable tax rate, as I do, is an important test for the validity of my empirical strategy.

1.5.2 2002 Policy Period Tax Refund Allocation

I next study how firms allocate their refund dollars. I look at each policy period separately so as not to assume that firms would have taken the same actions across the two periods. Table 5 shows the effect of the tax refund on potential uses of funds for the 2002 policy period (empirical specification 2). Panel A shows the effect on the major uses of funds on a firm's cash flow statement: investment, change in cash, change in total debt, payout, and other potential uses (acquisitions, short-term financial investment and long-term financial investment). Column 6 of Table 5 shows the tax refund's estimated effect on the total of these potential uses. The estimated coefficient in each of these regressions is interpreted as the marginal propensity of a firm to invest or use the funds otherwise out of each additional dollar of tax refund. Column 7 shows the

¹⁶ The highest corporate marginal tax rate in the United States is 35 percent, which phases in at taxable income of \$18.3 million. Any taxable income above \$75,000 is subject to a tax rate of at least 34 percent. The tax rate I estimate is the average rate paid on positive taxable income, not the average effective tax rate across firms. The average effective U.S. corporate tax rate is lower, because it averages firms that pay few taxes due to sustaining losses or due to having NOL carryforwards available to offset taxable profits. From 1996-2000, for example, total U.S. corporate tax receipts as a percentage of domestic economics profits averaged 25.9 percent. From 2003-2007, total U.S. corporate tax receipts as a percentage of domestic economics profits averaged 22.4 percent (CBO, 2014).

effect on the change in employment. The coefficient in this regression is interpreted as the change in the number of employees (in thousands) for each million dollars of tax refund received.

I find that in the 2002 policy period, firms allocated \$0.40 of each refund dollar to investment in the year they received the tax refund (2002 and 2003). I cannot identify a statistically significant average use of the rest of the funds and I find no effect of the refund on firm hiring. I find that the regression estimates across the major uses of funds sum to a point estimate of 1.09, indicating that \$1.09 of each \$1 of tax refund was allocated to one of these uses. This estimate is relatively close to \$1 of total uses, and shows that the empirical specification is capturing the uses of the tax refund reasonably well. In the years following receipt, I do not find any effects for the 2002 policy period (Appendix 3, Table A5).

Next, I study whether financial constraints and investment opportunities affected firms' responses to the tax refund policy. If firms were financially unconstrained, the tax refunds should have little effect on investment. As canonical theories in finance show, in a frictionless environment, a firm with attractive investment projects could receive necessary funding from capital markets. The result from the 2002 policy period is consistent with the hypothesis that, at the time, firms had profitable investment opportunities, but were financially constrained. When the five-year carryback policy lifted constraints, firms invested the funds. To test this hypothesis, I examine whether the investment increase was concentrated in financially constrained firms and whether it was concentrated in firms with better investment opportunities.

I separate firms into subsamples of financially constrained firms and unconstrained firms (and likewise into firms with high and low investment opportunities) and I compare the effects of tax refund receipt on investment between subsamples. I use three measures of financial constraints. First, I sort firms in the sample by the level of the Kaplan-Zingales (1997) index of financial constraints, and classify a firm as financially constrained if the firm falls in the top quartile of the

distribution.¹⁷ Second, I classify firms as constrained if they do not pay dividends or repurchase stock. Third, I use a Dharmapala, Foley and Forbes (2011) measure and define a firm as financially constrained if total payout to operating income is less than or equal to zero. All measures are calculated using lagged values of the firm characteristic.

To test whether the increase in capital expenditures was concentrated in firms with high investment opportunities, I classify a firm as having high investment opportunities in three ways: 1) if the firm falls in the top quartile of the distribution of lagged Tobin's q, 2) if the tax refund was received in 2003 (rather than 2002), and 3) if the firm is a "multinational" firm, defined as having a substantial share of foreign activity (foreign pre-tax income of more than 5 percent of total pre-tax income in absolute value as in Graham and Mills, 2008). By 2003, the economic recovery was gaining speed; it is therefore likely that firm investment opportunities were better in 2003 than in 2002. Multinational firms are also likely to have a larger investment opportunity set than domestic firms. I classify low investment opportunity firms, therefore, as: 1) those in the bottom quartile of Tobin's q, 2) as having received the refund in 2002, and 3) as having primarily domestic activity (foreign pre-tax income less than or equal to 5 percent of total pre-tax income in absolute value).

I present results for the effect of tax refund receipt on firm investment for financially constrained and unconstrained firms in Table 6, Panel A. As hypothesized, the investment result from the 2002 policy period is concentrated in financially constrained firms. I estimate that these firms spent between \$1.00 and \$1.09 of each \$1 of tax refund on investment. I see no statistically significant effect of the policy on the investment of unconstrained firms.

I also find evidence that investment spending out of the tax refund was concentrated in firms with higher investment opportunities (Table 6, Panel B): high Tobin's q firms (\$1.02 of each tax refund dollar, column 1), in 2003 (\$0.75 of each tax refund dollar, column 4), and in multinational firms (\$0.68 of each tax refund dollar, column 5). I find no statistically significant effect of tax refund

¹⁷ Using the Compustat variable names, this index is defined as: $-1.002 \cdot (IB + DP) / PPENT_{t-1} + 0.283 \cdot (AT + PRCC_C \cdot CSHO - CEQ - TXDB) / AT + 3.139 \cdot (DLTT + DLC) / (DLTT + SEQ) - 39.368 \cdot ((DVC + DVP) / PPENT_{t-1}) - 1.315 \cdot (CHE / PPENT_{t-1})$

receipt on investment in low Tobin's q firms, in 2002, or domestic firms (Columns 2, 3 and 6, respectively). Note that investment reported in Compustat is a worldwide measure of investment. Given that the investment response was concentrated in multinational firms, I cannot say for certain whether investment resulting from the refund policy was carried out in the United States as policymakers intended or whether firms transferred the tax refunds to invest overseas.¹⁸ My results represent an upper bound of the effect on domestic investment.

1.5.3 2009 Policy Period Tax Refund Allocation

In the 2009 policy period, I find that firms chose different uses of funds. In the year firms received the refund (2010), they allocated \$0.96 to higher cash holdings for every dollar of tax refund on average (Table 7, column 2). The regression estimates across all uses of the funds sum to a point estimate of 1.3 in this year, again suggesting that the specification is doing a reasonable job allocating \$1 of tax refund. Notably, I find no effect on firm investment in this period—even for financially constrained firms or firms with higher investment opportunities (Table A6)—and again, no effect on firm hiring. The cash effect is only marginally significant, however, with a p -value of 0.096. Note that because most firms receive just one tax refund in the 2009 period, the sample size in this period is much smaller than in the first policy period. The regression kink design is a relatively low power methodology (Card, Lee, Pei, and Weber, 2012) and a number of my regressions for the 2009 period appear to suffer from low power.

I find that in the year after receiving the refund (2011), firms reduced cash holdings and used the funds to pay down long-term debt. Firms reduced cash holdings by \$1.54 per dollar of tax refund in 2011 and reduced long-term debt in 2011 by \$1.26 of every tax refund dollar (Table 8, Panel A). I find no effects of the tax refunds in any following year. While these point estimates appear slightly high (they imply that for each dollar of tax refund, firms pay down more than a dollar in debt), the estimates are not statistically different from the cash increase estimate in Table 7 or

¹⁸ Firms report investment by geographic segment (domestic and non-domestic) in the Compustat Segments database, but the coverage appears poor in my sample. Of the 891 firm-years in the sample that report substantial foreign activity in this period, only 100 firms report foreign capital expenditures.

from \$1. Panel B of Table 8 shows results for the uses of the tax refunds over 2010 and 2011 in total. I find no average change in cash over the two years, consistent with firms first increasing and then paying down cash. The average effect over the two years was that firms paid down long-term debt in those years—\$1.36 of an additional tax refund dollar was allocated to debt reduction (significant at the 5 percent level).

Why did firms increase cash in the 2009 period? Theory points to two main hypotheses for the savings response: 1) holding cash due to higher idiosyncratic risk or uncertainty about future prospects in order to insulate the firm from future negative shocks (Opler, Pinkowitz, Stulz, and Williamson, 1999), and 2) holding cash when facing financing constraints in order to fund future investment opportunities (Almeida, Campello, and Weisbach, 2004; Han and Qiu, 2007). To test these hypotheses, I divide firms into subsamples based on proxies for high and low uncertainty about future prospects and I divide firms into samples based on measures of high and low financial constraints.

For the uncertainty test, I generate three proxies for uncertainty about future cash flows or performance by industry, following measures used in the literature: historical cash flow volatility, stock volatility and analyst forecast dispersion. The first proxy is historical cash flow volatility, constructed in the vein of Bates, Kahle, and Stulz (2009). I calculate the Fama-French 48 industry average of the 10-year standard deviation in firm cash flow/assets. For the stock return volatility measure, I calculate the industry average of the standard deviation of firm weekly stock returns over the past calendar year. For the analyst forecast dispersion proxy, I calculate the industry average of the standard deviation of analysts' earnings forecasts for the year forward (scaled by the prior year-end stock price). For each of these three measures, I classify firms in the top quartile of industry volatility or dispersion as the "high uncertainty" sample and firms in the bottom quartile as the "low uncertainty" sample. The stock volatility proxy and analyst forecast proxy are in the vein of Zhang (2006), but I use industry-level measures for consistency with the Bates,

Kahle and Stulz (2009) measure of cash-flow volatility. For the financial constraints test, I use the same measures of financial constraints as in Section 1.5.2 above.

I find that the cash holdings result is indeed concentrated in firms facing higher uncertainty as measured by past cash flow volatility and stock market volatility (Table 9, Panel A). The estimate for the cash change effect in the high cash flow volatility sample (column 1) is close to the point estimate from the main specification (1.09 versus 0.96) and is significant at the 1 percent level. The point estimate of the cash effect in the high stock volatility sample is somewhat high at 1.97 (column 3), though this estimate is not statistically different from a value of \$1. (Given the small size of these subsamples, additional noise may be expected.) For the subsample split by analyst forecast dispersion, there is no statistically significant effect on the change in cash for firms in either subsample, though the point estimate of the effect suggests that the cash effect is larger in the sample with higher analyst forecast dispersion (column 5).

Turning to the financial constraints hypothesis, I find no evidence that suggests the change in cash is concentrated in the most financially constrained firms. I see no statistically significant increase in cash in either the “high” or “low” financial constraint subsamples (Table 9, Panel B), suggesting that high financial constraints are not the primary motivation for firms holding the tax refunds as cash in 2010. I cannot rule out the fact that these may be poor measures of financial constraints in this period, however, given the tightness of credit conditions overall in the 2007-2009 recession, or that these may be poor measures of whether a firm expects to be financially constrained in the future.

1.5.4 Did Tax Refunds Improve Firm Financial Conditions?

Next, I examine how the tax refunds affected firm financial conditions. Another stated policy goal of the five-year tax carryback extension was helping firms “weather the storm.” I study six measures of firm financial conditions: three bankruptcy risk measures (Altman’s z-score, Ohlson’s o-score, and distance to default), two credit risk measures (the probabilities of a future S&P credit

rating upgrade or downgrade), and the probability of a future bankruptcy or liquidation. I describe these measures in Appendix 2.

Table 10 shows results for the bankruptcy risk and credit rating measures for the 2002 policy period (Panel A) and the 2009 policy period (Panel B). To provide economic magnitudes for the results, I present standardized coefficients that are interpreted as the standard deviation change in the outcome variable resulting from a one standard deviation change in the tax refund.

For the 2002 policy period, I see a small effect on bankruptcy risk: a statistically significant increase in Altman's z-score, which indicates a reduction in bankruptcy risk for a firm. A one standard deviation increase in a firm's tax refund results in about a tenth of a standard deviation increase in z-score on average in this sample. I see no statistically significant effect of the tax refunds on any of the other measures of bankruptcy risk or on firm credit ratings in this period, however.

I see larger effects for the 2009 policy period. A one standard deviation increase in the tax refund results in a 0.18 standard deviation increase in Altman's z-score, a smaller change in Ohlson's o-score (-0.05 standard deviation—note, a decrease in o-score indicates a reduction in bankruptcy risk), and about a fifth of a standard deviation decrease in distance to default one year and two years forward. Looking at the effect of the policy on a firm's credit risk, I see that it resulted in a statistically significant reduction in the probability of a credit rating downgrade. A one standard deviation increase in a firm's tax refund resulted in about a fifth of a standard deviation decrease in the probability of a credit rating downgrade over the next 24 months or 36 months. These findings are consistent with the results on the allocation of tax funds in the 2009 policy period: that firms held the tax refunds as cash first, and then reduced cash in order to pay down long-term debt. These financial decisions would tend to reduce firm riskiness overall and the riskiness of firm debt positions, in particular.

Finally, I look at whether a receiving a tax refund lowered the probability of a firm experiencing bankruptcy or liquidation (Table 11). I see no statistically significant effect, suggesting that while the tax refunds helped improve broad financial conditions in 2009, they did not stave off severe negative outcomes for firms. The incidence of bankruptcy in the sample is quite low overall, however, with only a few firms leaving the sample for this reason in the years after the policies were enacted.

1.5.5 Robustness

I carry out several robustness tests and show that my results are generally robust to the empirical specification and sample used, though the investment result for the 2002 policy period is more robust than the cash and debt results for the 2009 policy period. First, I conduct a “sharp RKD” test of the kink in the outcome variables. Second, I vary the order of the polynomial in V . Third, I narrow the bandwidth of the regression window. Fourth, I exclude industries that were particularly hard hit in each recession (telecommunications and airlines in 2002 and homebuilders in 2009). I present the results in Appendix 3.

I show results from a sharp RKD test of the main results in Table A1. From the 2002 policy period, I show the increase in investment (column 1) and in the 2009 policy period, I show the following results: the increase in cash in 2010 (column 2), the reductions in cash and long-term debt in 2011 (columns 3 and 4) and the two-year cumulative reduction in long-term debt in 2010 and 2011 (column 5). The sharp RKD strategy is estimated under empirical specification 1. As in the first-stage regression of the tax refund, the coefficient γ_1 recovers the change in slope of the outcome variable around the kink point. In the sharp RKD strategy, one estimates the change in slope of the outcome variable of interest (e.g., investment) with respect to the assignment

variable (V), and then divides by the change in slope of the policy variable (the average firm tax rate).¹⁹

Assuming a tax rate of 35 percent for all firms, the “sharp RKD” results are quite similar to the “fuzzy RKD” results for each of the variables. For example, the estimated change in the slope of investment in the 2002 period is 0.145, which corresponds to an estimate of \$0.41 per dollar of the tax refund spent on investment ($0.145/0.35$); this result is quite close to the fuzzy RKD estimate of \$0.40 in the baseline specification. In the first year of the 2009 period, the estimated slope change of the change in cash in the 2009 period is 0.31, which corresponds to an estimate of \$0.88 allocated to higher cash; this result is again quite close to the fuzzy RKD estimate of \$0.96. While the change in cash in 2010 is only marginally significant (p-value of 0.11), the other estimates are significant at the 5 percent level.

Table A2 shows results from tests that vary the polynomial order for the first-stage regression and the second-stage regression. I test 1) including only a linear interaction term with D, and 2) including the full polynomial interaction with D in the first-stage regression. I then test both first-stage options using a second-order and third-order polynomial in V in the second-stage regression. I report results for the same results as in the sharp RKD test. Each panel in the table reflects a separate regression. Each row shows results for a separate dependent variable and each column shows results for a different polynomial order.

My preferred regression specification uses a second-order polynomial in V with the second-order polynomial interacted with D also included in the first-stage excluded instrument. For the investment result, the estimated coefficient is fairly stable over a second- and third-order polynomial in V in the second stage. It is also stable using a linear polynomial term in the first-

¹⁹ This corresponds to the RKD estimand from expression A. The intuition for this calculation is that empirical specification (1) estimates the additional funds spent for each additional dollar of previous-years' profits on the left side of the kink point. The coefficient estimate in Table A1 suggests that firms spent an additional \$0.145 on investment for each \$1 of previous-years' profits in the 2002 period. But for each additional dollar of previous-years' profits, the firm did not receive an extra dollar of tax refund—the firm only received an extra \$0.35 of tax refund (if the tax rate was \$0.35). So the effect of the policy is that the firm spent \$0.145 out of the \$0.35 it received—\$0.145/\$0.35 or \$0.41 in total.

stage regression for both second-stage polynomial choices. Estimates range from \$0.39 cents per tax refund dollar spent on investment to \$0.47 per tax refund dollar. For the 2009 policy period, the regression coefficient on the cash reduction in the second year (2011) is also fairly stable over all specifications. For the other outcome variables, most regression coefficients are also in the general range of the preferred estimate. The majority of results remain statistically significant, though the 2010 cash change result in column 3 loses significance in the non-preferred specifications. In addition, specifications in column 2 (a linear term in the first stage and a second-order polynomial in the second stage) also lose significance. Including a second- or third-order polynomial interaction term in the first-stage regression appears reasonable as there may be unmeasured effects from only using a linear term. A linear term may not capture the tax refund function well for larger firms that are subject to the corporate Alternative Minimum Tax or have different uses of tax credits, for example.

Next, I test narrowing the regression bandwidth (Table A3). In the preferred specification, I use the full sample because the regression kink design is a relatively low power methodology (Card, Lee, Pei, and Weber, 2012). For the 2002 period, the estimated coefficient in the investment regression increases when narrowing the window (although not monotonically) and retains statistical significance for most specifications. Narrowing the bandwidth of the regression for the 2009 policy period, which reduces the sample size significantly, I see that the regression coefficients are not nearly as stable and I lose statistical significance in many windows. This result is consistent with Card, Lee, Pei, and Weber's result that RKD estimates tend to become noisy and lose power at lower bandwidths.

Finally, I exclude from the regression sample industries that experienced particularly large losses during the two recessions: telecom and airlines in 2002 and homebuilding in 2009 (Table A4). For the 2002 sample, I find that excluding these industries, firms increased investment by \$0.51 for every dollar of tax refund. This estimate is similar to the full-sample estimate (\$0.40) but slightly bigger, which is sensible because the excluded industries likely had poorer investment

opportunities at the time and would have been less likely to use the funds for investment. For the 2009 period, I find that the coefficient estimates on the changes in cash (columns 2 and 3) remain largely the same (\$1.04 for every refund dollar versus a \$0.96 baseline estimate in the first year and -\$1.48 versus -\$1.54 in the second year) though the statistical significance becomes marginal as the sample size declines. The estimates on the debt reductions in columns 4 and 5 are also of similar magnitudes as the baseline estimates and are statistically significant at the 5 percent level.

1.6 Conclusion

In this paper, I show that firm responses to a fiscal stimulus policy enacted in 2002 and in 2009 differed across the two periods. The policy I study granted additional tax refunds to firms by extending the carryback window for net operating losses. In the 2002 period, I find that firms used the tax refunds to increase investment in the year they received the refund. In the 2009 period, in contrast, I find that firms used the refunds to increase cash holdings in the year they received the refund. In the following year, firms decreased their cash holdings and used the funds to pay down long-term debt. I find that the tax refunds had an effect on improving firm financial conditions broadly in the 2009 period as well, lowering bankruptcy risk and lowering the probability of a credit rating downgrade, but I find fewer effects on financial health in 2002.

The contrasting results over the two periods are consistent with the hypothesis that firms may choose different uses of liquidity under different economic conditions, as shown in dynamic models of firm investment and financing policies such as Bolton, Chen and Wang (2011, 2013). Comparing economic conditions across the policy periods, in the 2002 period, the economy was recovering from a much milder recession. Growth prospects were higher and policy uncertainty was lower, though credit conditions remained tight. My finding that investment was concentrated in financially constrained firms and those with higher investment opportunities is consistent with the hypothesis that a number of firms had profitable investment opportunities at the time but were financially constrained. When the five-year carryback policy eased financial constraints in 2002

and 2003, therefore, these firms took advantage of the tax refunds to boost investment. The 2009 cash holdings result, on the other hand, was concentrated in firms facing higher uncertainty. This fact is consistent with a hypothesis that due to high economic uncertainty, an increase in cash due to a precautionary savings motive was the highest value use of funds at the time.

This work should be informative to policymakers considering implementing the five-year NOL carryback policy in the future. Is there evidence that the policy achieved the two goals of boosting investment and improving firm financial conditions? Yes, but my results suggest that the policy only achieved one of these goals in each period and I find no effect of the policy on employment—another stated policy goal. In addition, I cannot say for certain whether the policy boosted domestic investment in the 2002 period as policymakers desired. I measure an effect on worldwide investment and it was multinational firms in my sample that increased investment, not domestic firms. These firms may have transferred the funds overseas to invest.

This work also highlights the importance of policymakers carefully considering policy goals and broad economic conditions in evaluating the potential effectiveness of firm fiscal stimulus actions. If the main policy goal is to increase investment, for example, my results suggest that the carryback policy is more likely to be effective during a period when firms appear financially constrained but are not facing especially weak economic prospects or high levels of uncertainty. My results suggest that the policy is less likely to be effective increasing investment during a period of high uncertainty and when firms have weak investment opportunities. If the policy goal is to improve firm financial conditions broadly, however, my results suggest the carryback policy may indeed be effective during such times.

Figures and Tables

Figure 1: Example of Kink in the Tax Refund Formula

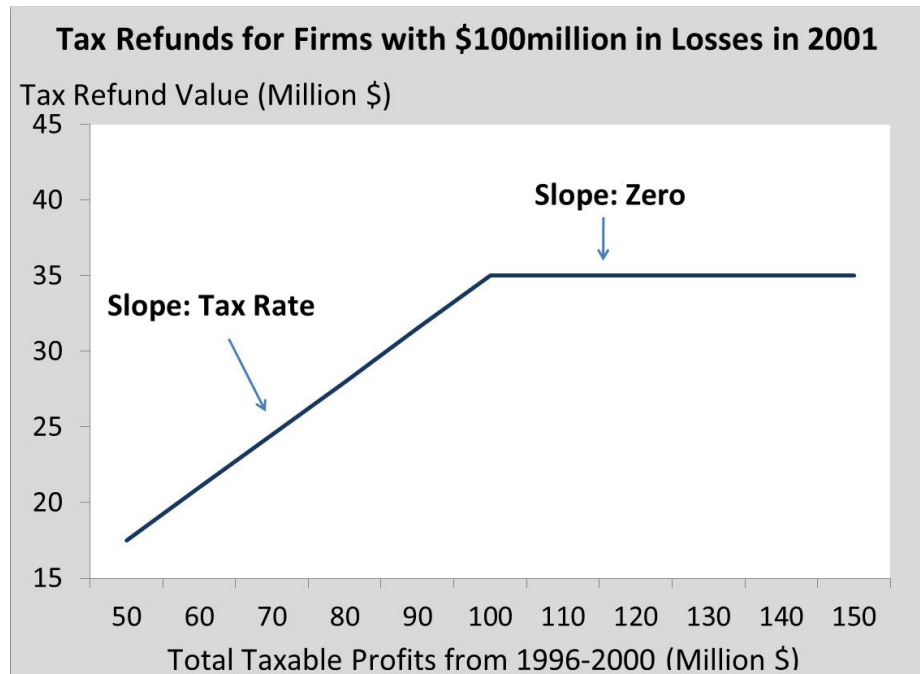


Figure 2: Distribution of sample firms around the kink point ($V=0$)

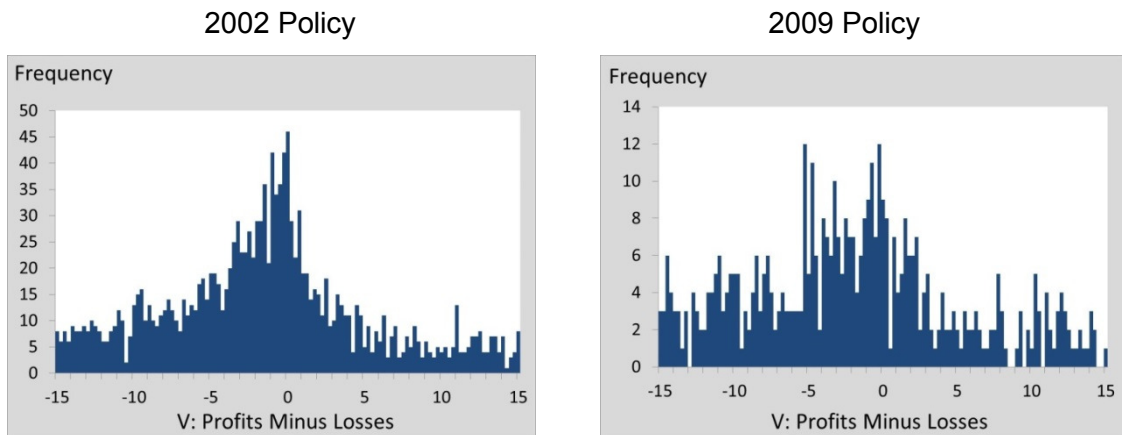


Figure 3: Distribution of pre-treatment firm characteristics around the kink point ($V = 0$) for the 2009 policy period

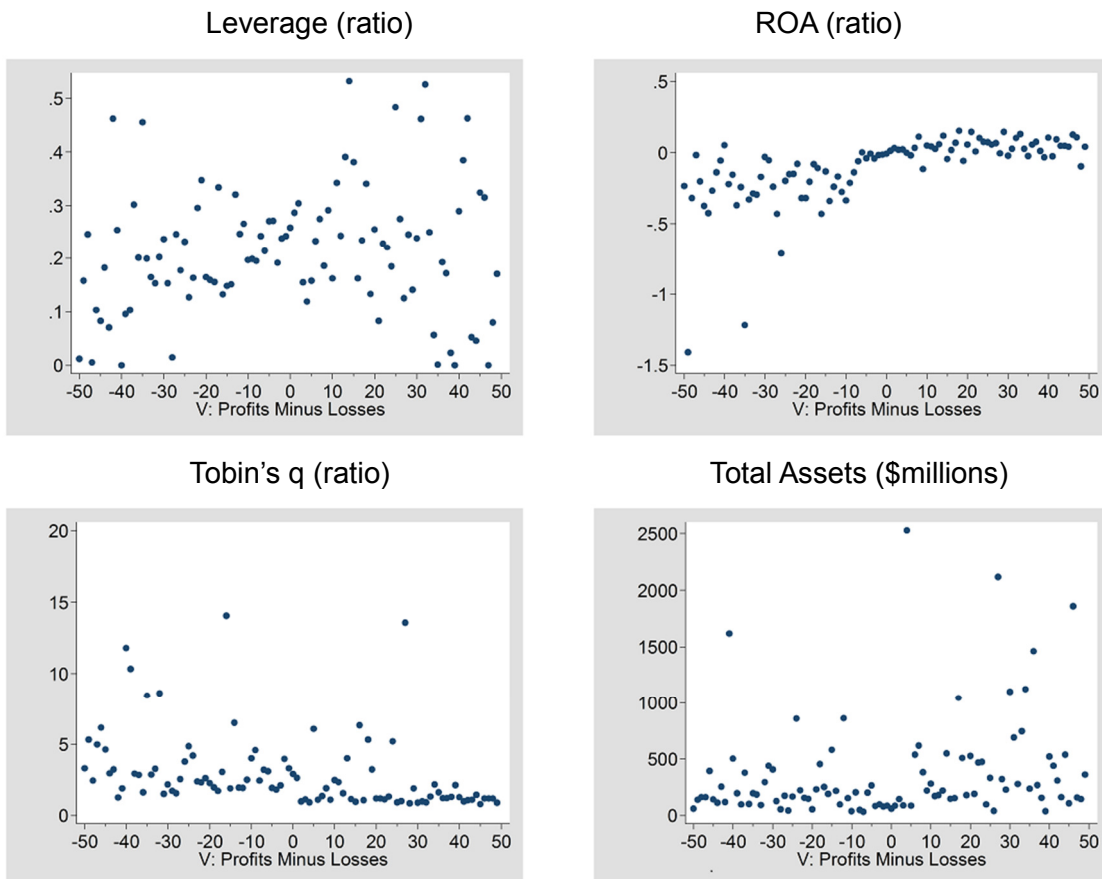
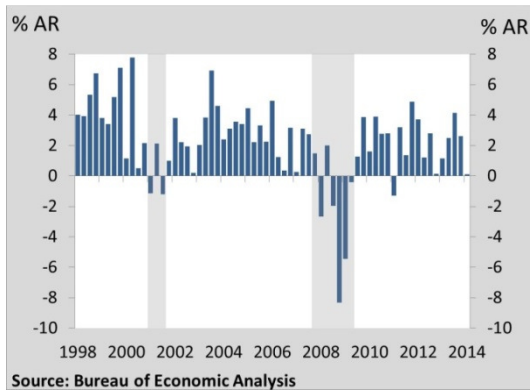
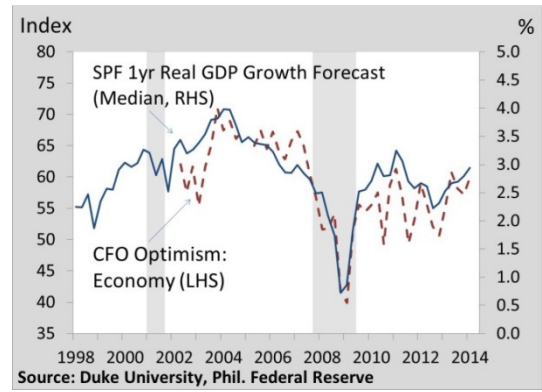


Figure 4: Economic Conditions: 2002 and 2009 Policy Period

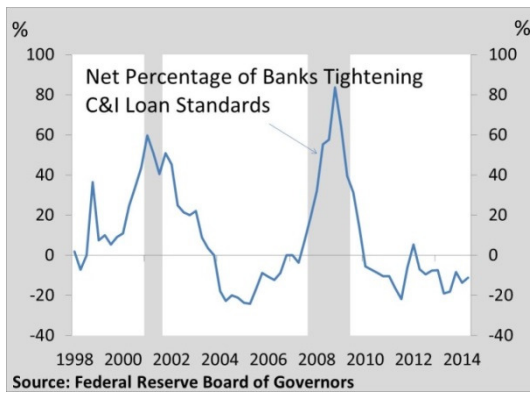
(A) Real GDP Growth



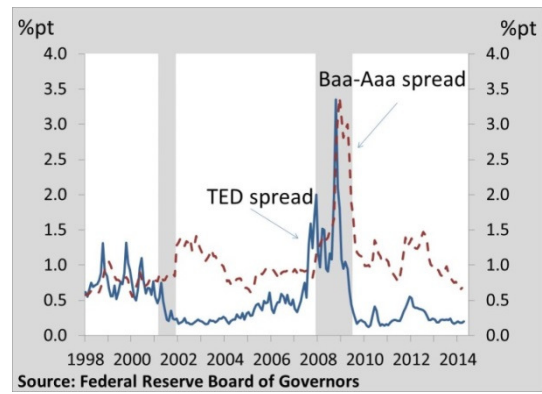
(B) Economic Outlook



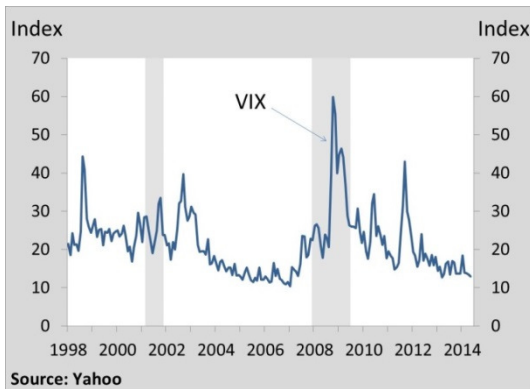
(C) Bank Lending Conditions



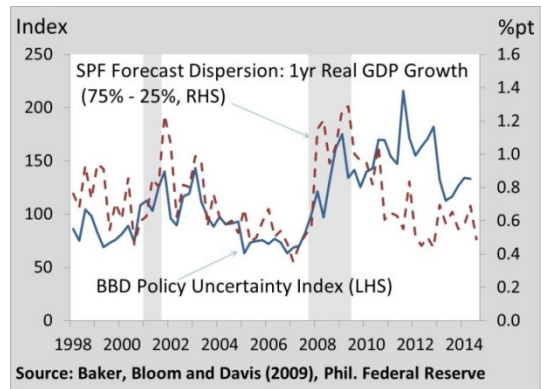
(D) Loan Spreads



(E) Stock Volatility



(F) Policy and Economic Uncertainty



Note: Gray shading represents recession periods

Table 1: NOL Carryback Deduction Example

	Tax Year					
	1996	1997	1998	1999	2000	2001
Before Carryback						
Taxable income before carryback (\$Mil)	50	40	30	20	10	-100
Taxes Paid, $\tau=0.35$ (\$Mil)	17.5	14.0	10.5	7.0	3.5	0
2-Year Carryback						
NOL carryback deduction (\$Mil)	0	0	0	-20	-10	30
Taxable income after carryback (\$Mil)	50	40	30	0	0	-70
Taxes Paid, $\tau=0.35$ (\$Mil)	17.5	14.0	10.5	0	0	0
Tax Refund (\$Mil)						10.5
5-Year Carryback						
NOL carryback deduction (\$Mil)	-50	-40	-10	0	0	100
Taxable income after carryback (\$Mil)	0	0	20	20	10	0
Taxes Paid, $\tau=0.35$ (\$Mil)	0	0	7.0	7.0	3.5	0
Tax Refund (\$Mil)						35

Table 2: Data Sample Summary Statistics

	2002 Policy		2009 Policy	
	Mean	Std. Dev	Mean	Std. Dev
V (Profits Minus Losses) (\$M)	11.27	290.13	28.95	480.43
Tax Refund (\$M)	8.84	44.70	12.51	50.70
Investment (\$M)	28.72	97.35	50.20	146.77
Change in Cash (\$M)	5.00	86.03	-2.10	171.99
Payout (\$M)	7.37	45.90	18.66	94.95
Change in Debt (\$M)	-11.04	198.70	-5.04	262.75
Change in Short-term Investments (\$M)	-0.30	64.76	5.14	85.35
Change in Long-term Investments (\$M)	23.70	225.99	38.40	244.39
Change in Employment (Thousands)	-0.17	1.98	0.07	3.21
Altman's z-score	-1.99	10.10	-2.60	11.89
Ohlson's o-score	1.63	8.55	0.47	6.73
Bankruptcy or Liquidation, 1yrF	0.00	0.03	0.00	0.03
Bankruptcy or Liquidation, 2yrF	0.01	0.07	0.00	0.06
S&P Credit Downgrade, 1yrF	0.26	0.44	0.14	0.35
S&P Credit Downgrade, 2yrF	0.35	0.48	0.20	0.40
S&P Credit Downgrade, 3yrF	0.43	0.50	0.26	0.44
S&P Credit Upgrade, 1yrF	0.13	0.34	0.27	0.44
S&P Credit Upgrade, 2yrF	0.24	0.43	0.36	0.48
S&P Credit Upgrade, 3yrF	0.32	0.47	0.47	0.50
Distance-to-default, 1yr	0.11	0.23	0.03	0.12
Distance-to-default, 2yr	0.11	0.24	0.03	0.12
Lagged ROA	-0.07	0.33	-0.07	0.40
Lagged ln(Assets)	4.87	1.96	5.28	2.03
Lagged Tobin's q	4.07	82.34	3.16	6.68
Lagged Cash Flow/Assets	-0.18	0.55	-0.15	0.70
Lagged Sales/Assets	1.10	1.09	1.03	1.01
Lagged Leverage	0.31	0.31	0.25	0.27
Lagged Marginal Tax Rate	0.20	0.11	0.21	0.11

The table reports summary statistics for outcome and control variables used in the analysis of the 2002 policy period (columns 1 and 2) and 2009 policy period (columns 3 and 4). Definitions of each variable are given in Appendix 2.

**Table 4: First-stage Regression Estimates:
Average Firm Tax Rate**

Dependent variable = Tax Refund		
	2002 Policy	2009 Policy
	(1)	(2)
Change in Slope	0.337*** [0.0356]	0.309*** [0.0514]
Controls	+	+
Industry F.E.	+	+
F-test (p-value)	0.00	0.00
Chi-squared test for coefficient differences (p-value)		0.71
Observations	3,337	1,496
R-squared	0.830	0.813

This table presents results from the first-stage regression, which estimates the change in slope of the tax refund as a function of the assignment variable around the kink point (the estimated coefficient γ_1 from empirical specification 1). The change in slope equals the estimated average tax rate across firms, as described in Section III. Columns 1 and 2 report the estimated tax rate for the 2002 period and the 2009 period, respectively. Regressions include a second-order polynomial in the assignment variable V , industry fixed effects at the Fama-French 48 level, and the pre-treatment controls Tobin's q , ROA, cash flow/assets, sales/assets, leverage, the marginal tax rate, $\ln(\text{assets})$, and a quadratic in stimulus losses. Standard errors are clustered at the Fama-French 48 industry level and are reported in brackets. ***, **, and * indicate levels of 1 percent, 5 percent, and 10 percent significance, respectively.

**Table 5: Tax Refund Allocation in the 2002 Policy Period:
Year of Tax Refund Receipt**

	Dependent variable =		Change in		Payout	Other Uses	Total	Change in Employment
	Investment	Cash	Total Debt					
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	
Tax Refund								
	0.403***	-0.189	-0.606	-0.0855	0.358	1.093	0.00271	
	[0.155]	[0.252]	[0.871]	[0.130]	[0.579]	[0.786]	[0.00711]	
Controls	+	+	+	+	+	+	+	
Industry F.E.	+	+	+	+	+	+	+	
Observations	3,337	3,337	3,337	3,337	3,337	3,337	3,232	
R-squared	0.404	0.073	0.050	0.145	0.056	0.235	0.066	

This table presents results from the second-stage regression (the coefficient β_1 from empirical specification 2) and estimates the effect of the tax refund on potential uses of the funds in the 2002 policy period in the year that the firms received the tax refund. Each column presents results from one regression; the dependent variable is listed at the top of each column. Other uses are acquisitions, change in short-term investments and change in investments. Total is the sum of all uses listed in columns 1 to 5. Regressions include a second-order polynomial in the assignment variable V , industry fixed effects at the Fama-French 48 level, and the pre-treatment controls Tobin's q , ROA, cash flow/assets, sales/assets, leverage, the marginal tax rate, $\ln(\text{assets})$, and a quadratic in stimulus losses. Standard errors are clustered at the industry level and are reported in brackets. ***, ** and * indicate levels of 1 percent, 5 percent, and 10 percent significance, respectively.

Table 6: Investment, Financial Constraints, and Investment Opportunities

Panel A: Financial Constraints

Dependent Variable = Investment

Financially Constrained?	KZ Index		Payout		DFF	
	Yes	No	Yes	No	Yes	No
	(1)	(2)	(3)	(4)	(5)	(6)
Tax Refund	1.091*	0.0534	1.004*	0.418	1.002**	0.501
	[0.659]	[0.228]	[0.535]	[0.307]	[0.509]	[0.389]
Controls	+	+	+	+	+	+
Industry F.E.	+	+	+	+	+	+
Observations	785	787	2,196	1,141	2,501	816
R-squared	0.571	0.544	0.413	0.453	0.402	0.465

Panel B: Investment Opportunities

Dependent Variable = Investment

High Investment Opportunities?	Tobin's Q		Year of Tax Refund		Multi-national	
	High	Low	2002	2003	national	Domestic
	Yes	No	No	Yes	Yes	No
	(1)	(2)	(3)	(4)	(5)	(6)
Tax Refund	1.017*	-1.454	0.277	0.753***	0.676*	0.171
	[0.593]	[1.481]	[0.384]	[0.243]	[0.372]	[0.177]
Controls	+	+	+	+	+	+
Industry F.E.	+	+	+	+	+	+
Observations	833	835	1727	1610	891	2434
R-squared	0.500	0.371	0.395	0.428	0.441	0.419

This table presents results from the second-stage regression (the coefficient β_1 from empirical specification 2) and estimates the effect of the tax refund on investment in the 2002 policy period in subsamples of financially constrained and unconstrained firms and firms with high and low investment opportunities. Panel A restricts the sample by three measures of firm financial constraints. Columns 1 and 2 show results for subsamples of firms in the top and bottom quartiles of the Kaplan-Zingales (1997) index, respectively. Columns 3 and 4 show results for subsamples of firms with zero and non-zero dividend issuance and stock repurchases, respectively. Columns 5 and 6 show results for subsamples of firms for which total payouts to operating income is and is not less than or equal to zero, respectively, as in Dharmapala, Foley, and Forbes (2011). Panel B restricts the sample by three measures of firm investment opportunities. Columns 1 and 2 show results for subsamples of firms in the top and bottom quartiles of Tobin's q, respectively. Columns 3 and 4 show results for tax refund receipt in 2002 and 2003, respectively. Columns 5 and 6 show results for firms with substantial foreign activity (foreign pre-tax income greater than 5 percent of total pre-tax income in absolute value) and domestic firms, respectively. All financial constraint and investment opportunity measures are calculated for the year prior to tax refund receipt. Regressions include a second-order polynomial in the assignment variable V, industry fixed effects at the Fama-French 48 level, and the pre-treatment controls Tobin's q, ROA, cash flow/assets, sales/assets, leverage, the marginal tax rate, $\ln(\text{assets})$, and a quadratic in stimulus losses. Standard errors are clustered at the industry level and are reported in brackets. ***, ** and * indicate levels of 1 percent, 5 percent, and 10 percent significance, respectively.

**Table 7: Tax Refund Allocation in the 2009 Policy Period:
Year of Tax Refund Receipt**

Table 5: Tax Refund Receipts									
	Dependent variable = Investment		Change in Cash		Change in Total Debt	Payout	Other Uses	Total	Change in Employment
	(1)	(2)	(3)	(4)	(5)	(6)	(7)		
Tax Refund	-0.406 [0.380]	0.958* [0.576]	-0.472 [0.386]	0.313 [0.496]	-0.0257 [1.000]	1.312 [1.916]	-0.00481 [0.00923]		
Controls	+	+	+	+	+	+	+		
Industry F.E.	+	+	+	+	+	+	+		
Observations	1,496	1,496	1,496	1,496	1,496	1,496	1,438		
R-squared	0.395	0.266	0.171	0.215	0.143	0.328	0.183		

This table presents results the second-stage regression (the coefficient β_1 from empirical specification 2) and estimates the effect of the tax refund on potential uses of the funds in the first year of tax refund receipt in the 2009 policy period. Other uses are acquisitions, change in short-term investments and change in investments. Total is the sum of all uses listed in columns 1 to 5. Regressions include a second-order polynomial in the assignment variable V , industry fixed effects at the Fama-French 48 level, and the pre-treatment controls Tobin's q , ROA, cash flow/assets, sales/assets, leverage, the marginal tax rate, $\ln(\text{assets})$, and a quadratic in stimulus losses. Standard errors are clustered at the industry level and are reported in brackets. ***, ** and * indicate levels of 1 percent, 5 percent, and 10 percent significance, respectively.

Table 8: Tax Refund Allocation in the 2009 Policy Period**Panel A: Year After Tax Refund Receipt (2011)**

Dependent variable =	Investment	Change in Cash	Change in Long-Term Debt	Change in Short-Term Debt	Payout	Other Uses	Total
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Tax Refund	-0.632 [0.542]	-1.542* [0.828]	-1.260** [0.626]	0.0568 [0.534]	0.124 [0.680]	-0.0972 [1.545]	-0.943 [2.610]
Controls	+	+	+	+	+	+	+
Industry F.E.	+	+	+	+	+	+	+
Observations	1,355	1,355	1,355	1,355	1,355	1,355	1,355
R-squared	0.363	0.026	0.076	0.079	0.245	0.129	0.254

Panel B: Two-Year Total Effect After Tax Refund Receipt (2010 and 2011)

Dependent variable =	Investment	Change in Cash	Change in Long-Term Debt	Change in Short-Term Debt	Payout	Other Uses	Total
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Tax Refund	-1.008 [0.887]	-0.524 [0.968]	-1.361** [0.622]	-0.363 [0.477]	0.419 [1.174]	-0.317 [2.532]	0.295 [4.593]
Controls	+	+	+	+	+	+	+
Industry F.E.	+	+	+	+	+	+	+
Observations	1,355	1,355	1,355	1,355	1,355	1,355	1,355
R-squared	0.384	0.150	0.114	0.378	0.254	0.154	0.307

This table presents the coefficient β_1 from empirical specification (2), the second-stage regression, and estimates the effect of the tax refund on potential uses of the funds in the 2009 policy period. Panel A reports estimates for the year following tax refund receipt (2011) and Panel B reports estimates for the cumulative use of funds over 2010 and 2011. Other uses are acquisitions, change in short-term investments and change in investments. Total is the sum of all uses listed in columns 1 to 6. Regressions include a second-order polynomial in the assignment variable "V", industry fixed effects at the Fama-French 48 level, and the pre-treatment controls Tobin's q, ROA, cash flow/assets, sales/assets, leverage, the marginal tax rate, $\ln(\text{assets})$, and a quadratic in stimulus losses. Standard errors are clustered at the industry level and are reported in brackets. ***, ** and * indicate levels of 1 percent, 5 percent, and 10 percent significance, respectively

Table 9: Change in Cash, Uncertainty, and Financial Constraints**Panel A: Uncertainty Proxies**

Dependent Variable = Change in Cash

Volatility?	Cash Flow Volatility		Stock Volatility		Analyst Forecast	
	High	Low	High	Low	High	Low
	(1)	(2)	(3)	(4)	(5)	(6)
Tax Refund	1.061*** [0.266]	-0.113 [0.505]	1.972* [1.015]	0.382 [0.843]	0.909 [1.193]	-0.748 [1.048]
Controls	+	+	+	+	+	+
Industry F.E.	+	+	+	+	+	+
Observations	295	467	342	525	352	461
R-squared	0.678	0.548	0.251	0.078	0.368	0.205

Panel B: Financial Constraints

Dependent Variable = Change in Cash

Financially Constrained	KZ Index		Payout		DFF	
	Yes	No	Yes	No	Yes	No
	(1)	(2)	(3)	(4)	(5)	(6)
Tax Refund	-0.484 [0.520]	0.535 [1.254]	0.0661 [0.701]	0.749 [0.790]	0.359 [0.705]	0.951 [0.766]
Controls	+	+	+	+	+	+
Industry F.E.	+	+	+	+	+	+
Observations	355	356	939	557	1,087	406
R-squared	0.162	0.492	0.570	0.213	0.541	0.236

This table presents the coefficient β_1 from empirical specification (2), the second-stage regression, and estimates the effect of the tax refund on the change in cash. Panel A restricts the sample by three proxies for uncertainty: firms in the top and bottom quartiles of Fama-French 48 industries based on the standard deviation of 10-year cash flow volatility (columns 1 and 2, respectively), top and bottom industry quartiles of the standard deviation of 1-year stock returns (columns 3 and 4) and top and bottom industry quartiles of the dispersion of analyst year-ahead earnings per share forecasts (columns 5 and 6). Panel B restricts the sample by three measures of firm financial constraints. Columns 1 and 2 show results for subsamples of firms in the top and bottom quartiles of the Kaplan-Zingales (1997) index, respectively. Columns 3 and 4 show results for subsamples of firms with zero and non-zero dividend issuance and repurchases, respectively. Columns 5 and 6 show results for subsamples of firms for which total payouts to operating income is and is not less than or equal to zero, respectively, as in Dharmapala, Foley, and Forbes (2011). All uncertainty and financial constraint measures are calculated for the year prior to tax refund receipt. Regressions include a second-order polynomial in the assignment variable "V", industry fixed effects at the Fama-French 48 level, and the pre-treatment controls Tobin's q, ROA, cash flow/assets, sales/assets, leverage, the marginal tax rate, $\ln(\text{assets})$, and a quadratic in stimulus losses. Standard errors are clustered at the industry level and are reported in brackets. ***, ** and * indicate levels of 1 percent, 5 percent, and 10 percent significance, respectively.

Table 10: Effect of the Tax Refund on Firm Financial Conditions

Panel A: 2002 Policy

	Bankruptcy Risk				S&P Credit Rating Downgrade			S&P Credit Rating Upgrade			
	Altman's Z-score		Ohlson's O-score	Distance-to-Default (1yr)	Distance-to-Default (2yr)	(12mF)	(24mF)	(36mF)	(12mF)	(24mF)	(36mF)
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	
Dependent variable =											
Tax Refund											
	0.151*	-0.0106	-0.0225	-0.0285	0.103	0.0927	0.092	0.00423	-0.0593	-0.0647	
	[0.0911]	[0.0471]	[0.0559]	[0.0520]	[0.0696]	[0.0659]	[0.0802]	[0.0759]	[0.0862]	[0.0818]	
Controls	+	+	+	+	+	+	+	+	+	+	
Industry F.E.	+	+	+	+	+	+	+	+	+	+	
Observations	3,276	3,290	2,057	2,057	747	754	768	744	751	756	
R-squared	0.296	0.139	0.248	0.235	0.116	0.115	0.121	0.116	0.101	0.114	

Panel B: 2009 Policy

	Bankruptcy Risk			S&P Credit Rating Downgrade			S&P Credit Rating Upgrade		
	Altman's Z-score	Ohlson's O-score	Distance-to-Default	(12mF)	(24mF)	(36mF)	(12mF)	(24mF)	(36mF)
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
	(10)	(11)	(12)	(13)	(14)	(15)	(16)	(17)	(18)
	(19)	(20)	(21)	(22)	(23)	(24)	(25)	(26)	(27)
	(28)	(29)	(30)	(31)	(32)	(33)	(34)	(35)	(36)
	(37)	(38)	(39)	(40)	(41)	(42)	(43)	(44)	(45)
	(46)	(47)	(48)	(49)	(50)	(51)	(52)	(53)	(54)
	(55)	(56)	(57)	(58)	(59)	(60)	(61)	(62)	(63)
	(64)	(65)	(66)	(67)	(68)	(69)	(70)	(71)	(72)
	(73)	(74)	(75)	(76)	(77)	(78)	(79)	(80)	(81)
	(82)	(83)	(84)	(85)	(86)	(87)	(88)	(89)	(90)
	(91)	(92)	(93)	(94)	(95)	(96)	(97)	(98)	(99)
	(100)	(101)	(102)	(103)	(104)	(105)	(106)	(107)	(108)
	(109)	(110)	(111)	(112)	(113)	(114)	(115)	(116)	(117)
	(118)	(119)	(120)	(121)	(122)	(123)	(124)	(125)	(126)
	(127)	(128)	(129)	(130)	(131)	(132)	(133)	(134)	(135)
	(136)	(137)	(138)	(139)	(140)	(141)	(142)	(143)	(144)
	(145)	(146)	(147)	(148)	(149)	(150)	(151)	(152)	(153)
	(154)	(155)	(156)	(157)	(158)	(159)	(160)	(161)	(162)
	(163)	(164)	(165)	(166)	(167)	(168)	(169)	(170)	(171)
	(172)	(173)	(174)	(175)	(176)	(177)	(178)	(179)	(180)
	(181)	(182)	(183)	(184)	(185)	(186)	(187)	(188)	(189)
	(190)	(191)	(192)	(193)	(194)	(195)	(196)	(197)	(198)
	(199)	(200)	(201)	(202)	(203)	(204)	(205)	(206)	(207)
	(208)	(209)	(210)	(211)	(212)	(213)	(214)	(215)	(216)
	(217)	(218)	(219)	(220)	(221)	(222)	(223)	(224)	(225)
	(226)	(227)	(228)	(229)	(230)	(231)	(232)	(233)	(234)
	(235)	(236)	(237)	(238)	(239)	(240)	(241)	(242)	(243)
	(244)	(245)	(246)	(247)	(248)	(249)	(250)	(251)	(252)
	(253)	(254)	(255)	(256)	(257)	(258)	(259)	(260)	(261)
	(262)	(263)	(264)	(265)	(266)	(267)	(268)	(269)	(270)
	(271)	(272)	(273)	(274)	(275)	(276)	(277)	(278)	(279)
	(280)	(281)	(282)	(283)	(284)	(285)	(286)	(287)	(288)
	(289)	(290)	(291)	(292)	(293)	(294)	(295)	(296)	(297)
	(298)	(299)	(300)	(301)	(302)	(303)	(304)	(305)	(306)
	(307)	(308)	(309)	(310)	(311)	(312)	(313)	(314)	(315)
	(316)	(317)	(318)	(319)	(320)	(321)	(322)	(323)	(324)
	(325)	(326)	(327)	(328)	(329)	(330)	(331)	(332)	(333)
	(334)	(335)	(336)	(337)	(338)	(339)	(340)	(341)	(342)
	(343)	(344)	(345)	(346)	(347)	(348)	(349)	(350)	(351)
	(352)	(353)	(354)	(355)	(356)	(357)	(358)	(359)	(360)
	(361)	(362)	(363)	(364)	(365)	(366)	(367)	(368)	(369)
	(370)	(371)	(372)	(373)	(374)	(375)	(376)	(377)	(378)
	(379)	(380)	(381)	(382)	(383)	(384)	(385)	(386)	(387)
	(388)	(389)	(390)	(391)	(392)	(393)	(394)	(395)	(396)
	(397)	(398)	(399)	(400)	(401)	(402)	(403)	(404)	(405)
	(406)	(407)	(408)	(409)	(410)	(411)	(412)	(413)	(414)
	(415)	(416)	(417)	(418)	(419)	(420)	(421)	(422)	(423)
	(424)	(425)	(426)	(427)	(428)	(429)	(430)	(431)	(432)
	(433)	(434)	(435)	(436)	(437)	(438)	(439)	(440)	(441)
	(442)	(443)	(444)	(445)	(446)	(447)	(448)	(449)	(450)
	(451)	(452)	(453)	(454)	(455)	(456)	(457)	(458)	(459)
	(460)	(461)	(462)	(463)	(464)	(465)	(466)	(467)	(468)
	(469)	(470)	(471)	(472)	(473)	(474)	(475)	(476)	(477)
	(478)	(479)	(480)	(481)	(482)	(483)	(484)	(485)	(486)
	(487)	(488)	(489)	(490)	(491)	(492)	(493)	(494)	(495)
	(496)	(497)	(498)	(499)	(500)	(501)	(502)	(503)	(504)
	(505)	(506)	(507)	(508)	(509)	(510)	(511)	(512)	(513)
	(514)	(515)	(516)	(517)	(518)	(519)	(520)	(521)	(522)
	(523)	(524)	(525)	(526)	(527)	(528)	(529)	(530)	(531)
	(532)	(533)	(534)	(535)	(536)	(537)	(538)	(539)	(540)
	(541)	(542)	(543)	(544)	(545)	(546)	(547)	(548)	(549)
	(550)	(551)	(552)	(553)	(554)	(555)	(556)	(557)	(558)
	(559)	(560)	(561)	(562)	(563)	(564)	(565)	(566)	(567)
	(568)	(569)	(570)	(571)	(572)	(573)	(574)	(575)	(576)
	(577)	(578)	(579)	(580)	(581)	(582)	(583)	(584)	(585)
	(586)	(587)	(588)	(589)	(590)	(591)	(592)	(593)	(594)
	(595)	(596)	(597)	(598)	(599)	(600)	(601)	(602)	(603)
	(604)	(605)	(606)	(607)	(608)	(609)	(610)	(611)	(612)
	(613)	(614)	(615)	(616)	(617)	(618)	(619)	(620)	(621)
	(622)	(623)	(624)	(625)	(626)	(627)	(628)	(629)	(630)
	(631)	(632)	(633)	(634)	(635)	(636)	(637)	(638)	(639)
	(640)	(641)	(642)	(643)	(644)	(645)	(646)	(647)	(648)
	(649)	(650)	(651)	(652)	(653)	(654)	(655)	(656)	(657)
	(658)	(659)	(660)	(661)	(662)	(663)	(664)	(665)	(666)
	(667)	(668)	(669)	(670)	(671)	(672)	(673)	(674)	(675)
	(676)	(677)	(678)	(679)	(680)	(681)	(682)	(683)	(684)
	(685)	(686)	(687)	(688)	(689)	(690)	(691)	(692)	(693)
	(694)	(695)	(696)	(697)	(698)	(699)	(700)	(701)	(702)
	(703)	(704)	(705)	(706)	(707)	(708)	(709)	(710)	(711)
	(712)	(713)	(714)	(715)	(716)	(717)	(718)	(719)	(720)
	(721)	(722)	(723)	(724)	(725)	(726)	(727)	(728)	(729)
	(730)	(731)	(732)	(733)	(734)	(735)	(736)	(737)	(738)
	(739)	(740)	(741)	(742)	(743)	(744)	(745)	(746)	(747)
	(748)	(749)	(750)	(751)	(752)	(753)	(754)	(755)	(756)
	(757)	(758)	(759)	(760)	(761)	(762)	(763)	(764)	(765)
	(766)	(767)	(768)	(769)	(770)	(771)	(772)	(773)	(774)
	(775)	(776)	(777)	(778)	(779)	(780)	(781)	(782)	(783)
	(784)	(785)	(786)	(787)	(788)	(789)	(790)	(791)	(792)
	(793)	(794)	(795)	(796)	(797)	(798)	(799)	(800)	(801)
	(802)	(803)	(804)	(805)	(806)	(807)	(808)	(809)	(810)
	(811)	(812)	(813)	(814)	(815)	(816)	(817)	(818)	(819)
	(820)	(821)	(822)	(823)	(824)	(825)	(826)	(827)	(828)
	(829)	(830)	(831)	(832)	(833)	(834)	(835)	(836)	(837)
	(838)	(839)	(840)	(841)	(842)	(843)	(844)	(845)	(846)
	(847)	(848)	(849)	(850)	(851)	(852)	(853)	(854)	(855)
	(856)	(857)	(858)	(859)	(860)	(861)	(862)	(863)	(864)
	(865)	(866)	(867)	(868)	(869)	(870)	(871)	(872)	(873)
	(874)	(875)	(876)	(877)	(878)	(879)	(880)	(881)	(882)
	(883)	(884)	(885)	(886)	(887)	(888)	(889)	(890)	(891)
	(892)	(893)	(894)	(895)	(896)	(897)	(898)	(899)	(900)
	(901)	(902)	(903)	(904)	(905)	(906)	(907)	(908)	(909)
	(910)	(911)	(912)	(913)	(914)	(915)	(916)	(917)	(918)
	(919)	(920)	(921)	(922)	(923)	(924)	(925)	(926)	(927)
	(928)	(929)	(930)	(931)	(932)	(933)	(934)	(935)	(936)
	(937)	(938)	(939)	(940)	(941)	(942)	(943)	(944)	(945)
	(946)	(947)	(948)	(949)	(950)	(951)	(952)	(953)	(954)
	(955)	(956)	(957)	(958)	(959)	(960)	(961)	(962)	(963)
	(964)	(965)	(966)	(967)	(968)	(969)	(970)	(971)	(972)
	(973)	(974)	(975)	(976)	(977)	(978)	(979)	(980)	(981)
	(982)	(983)	(984)	(985)	(986)	(987)	(988)	(989)	(990)
	(991)	(992)	(993)	(994)	(995)	(996)	(997)	(998)	(999)
	(1000)	(1001)	(1002)	(1003)	(1004)	(1005)	(1006)	(1007)	(1008)
	(1009)	(1010)	(1011)	(1012)	(1013)	(1014)	(1015)	(1016)	(1017)
	(1018)	(1019)	(1020)	(1021)	(1022)	(1023)	(1024)	(1025)	(1026)
	(1027)	(1028)	(1029)	(1030)	(1031)	(1032)	(1033)	(1034)	(1035)
	(1036)	(1037)	(1038)	(1039)	(1040)	(1041)	(1042)	(1043)	(1044)
	(1045)	(1046)	(1047)	(1048)	(1049)	(1050)	(1051)	(1052)	(1053)
	(1054)	(1055)	(1056)	(1057)	(1058)	(1059)	(1060)	(1061)	(1062)
	(1063)	(1064)	(1065)	(1066)	(1067)	(1068)	(1069)	(1070)	(1071)
	(1072)	(1073)	(1074)	(1075)	(1076)	(1077)	(1078)	(1079)	(1080)
	(1081)	(1082)	(1083)	(1084)	(1085)	(1086)	(1087)	(1088)	(1089)
	(1090)	(1091)	(1092)	(1093)	(1094)	(1095)	(1096)	(1097)	(1098)
	(1099)	(1100)	(1101)	(1102)	(1103)	(1104)	(1105)	(1106)	(1107)
	(1108)	(1109)	(1110)	(1111)	(1112)	(1113)	(1114)	(1115)	(1116)
	(1117)	(1118)	(1119)	(1120)	(1121)	(1122)	(1123)	(1124)	(1125)
	(1126)	(1127)	(1128)	(1129)	(1130)	(1131)	(1132)	(1133)	(1134)
	(1135)	(1136)	(1137)	(1138)	(1139)	(1140)	(1141)	(1142)	(1143)
	(1144)	(1145)	(1146)	(1147)	(1148)	(1149)	(1150)	(1151)	(1152)
	(1153)	(1154)	(1155)	(1156)	(1157)	(1158)	(1159)	(1160)	(1161)
	(1162)	(1163)	(1164)	(1165)	(1166)	(1167)	(1168)	(1169)	(1170)
	(1171)	(1172)	(1173)	(1174)	(1175)	(1176)	(1177)	(1178)	(1179)
	(1180)	(1181)	(1182)	(1183)	(1184)	(1185)	(1186)	(1187)	(1188)
	(1189)	(1190)	(1191)	(1192)	(1193)	(1194)	(1195)	(1196)	(1197)
	(1198)	(1199)	(1200)	(1201)	(1202)	(1203)	(1204)	(1205)	(1206)
	(1207)	(1208)	(1209)	(1210)	(1211)	(1212)	(1213)	(1214)	(1215)
	(1216)	(1217)	(1218)	(1219)	(1220)	(1221)	(1222)	(1223)	(1224)
	(1225)	(1226)	(1227)	(1228)	(1229)	(1230)	(1231)	(1232)	(1233)
	(1234)	(1235)	(1236)	(1237)	(1238)	(1239)	(1240)	(1241)	(1242)
	(1243)	(1244)	(1245)	(1246)	(1247)	(1248)	(1249)	(1250)	(1251)
	(1252)	(1253)	(1254)	(1255)	(1256)	(1257)	(1258)	(1259)	(1260)
	(1261)	(1262)	(1263)	(1264)	(1265)	(1266)	(1267)	(1268)	(1269)
	(1270)	(1271)	(1272)	(1273)	(1274)	(1275)	(1276)	(1277)	(1278)
	(1279)	(1280)	(1281)	(1282)	(1283)	(1284)	(1285)	(1286)	(1287)
	(1288)	(1289)	(1290)	(1291)	(1292)	(1293)	(1294)	(1295)	(1296)
	(1297)	(1298)	(1299)	(1300)	(1301)	(1302)	(1303)	(1304)	(1305)
	(1306)	(1307)	(1308)	(1309)	(1310)	(1311)	(1312)	(1313)	(1314)
	(1315)	(1316)	(1317)	(1318)	(1319)	(1320)	(1321)	(1322)	(1323)
	(1324)	(1325)	(1326)	(1327)	(1328)	(1329)	(1330)	(1331)	(1332)
	(1333)	(1334)	(1335)	(1336)	(1337)	(1338)	(1339)	(1340)	(1341)
	(1342)	(1343)	(1344)	(1345)	(1346)	(1347)	(1348)	(1349)	(1350)

This table presents the coefficient β_1 from empirical specification (2), the second-stage regression, and estimates the effect of the tax refund on measures of bankruptcy risk (Altman's z-score, Ohlson's o-score and distance-to-default in columns 1 to 4) and the probability of an S&P credit rating upgrade or downgrade (in columns 5 to 10). Panel A presents results from the 2002 policy period and Panel B presents results from the 2009 policy period. The coefficients are standardized to be interpreted as the standard deviation change in the dependent variable resulting from a one standard deviation change in the firm tax refund. Regressions include a second-order polynomial in the assignment variable "v", industry fixed effects at the Fama-French 48 level, and the pre-treatment controls Tobin's q, ROA, cash flow/assets, sales/assets, leverage, the marginal tax rate, ln(assets), and a quadratic in stimulus losses. Standard errors are clustered at the industry level and are reported in brackets. ***, **, and * indicate levels of 1 percent, 5 percent, and 10 percent significance, respectively.

Table 11: Effect of Tax Refund Receipt on Probability of Bankruptcy or Liquidation

Dependent Variable = Indicator for Bankruptcy or Liquidation

	2002 Policy		2009 Policy	
	12mF	24mF	12mF	24mF
	(1)	(2)	(3)	(4)
Tax Refund	-0.106 [0.125]	-0.0585 [0.0675]	0.00669 [0.00905]	-0.136 [0.108]
Controls	+	+	+	+
Industry F.E.	+	+	+	+
Observations	3,385	3,385	1,518	1,518
R-squared	0.003	0.018	0.015	0.025

This table presents the coefficient β_1 from empirical specification (2), the second-stage regression, and shows the effect of tax refund receipt on the probability of future bankruptcy or liquidation. The variables are standardized to be interpreted as the standard deviation change in the dependent variable resulting from a one standard deviation change in the firm tax refund. Regressions include a second-order polynomial in the assignment variable "V", industry fixed effects at the Fama-French 48 level, and the pre-treatment controls Tobin's q, ROA, cash flow/assets, sales/assets, leverage, the marginal tax rate, $\ln(\text{assets})$, and a quadratic in stimulus losses. Standard errors are clustered at the Fama-French 48 industry level and are reported in brackets. ***, **, and * indicate levels of 1 percent, 5 percent, and 10 percent significance, respectively.

CHAPTER 2: State-Dependent Effects of Consumer Credit: The Payday Lending Case

2.1 Introduction

U.S. households are heavy users of credit. There was \$13.2 trillion in household debt outstanding in 2010—about equal to total U.S. gross domestic product in that year. Seventy-seven percent of households held some form of debt, with the largest share of families holding mortgage debt (48.7 percent), followed by installment debt (46.9 percent) and credit card balances (46.1 percent). Debt payments represent a considerable fraction of household income as well. The median ratio of debt payments to family income was 18 percent for households holding debt in 2010.²⁰ Such high levels of household debt have tended to attract negative attention from the public and the media. But is credit access truly harmful to households and the economy?

Economic theory suggests the effects on well-being are instead likely to be heterogeneous and state-dependent. On one hand, canonical economic models of consumer credit show that credit access improves household utility by allowing users to smooth consumption over income fluctuations or other negative shocks. On the other hand, when individuals have an unusually strong preference for current consumption—problems of “self control” when it comes to consumption—credit access can lower household utility because households borrow to excess (Laibson, 1997; O’Donoghue and Rabin, 1999; Heidhues and Koszegi, 2010). In addition, credit access may lower well-being for some borrowers due to asymmetric information between lenders and borrowers, either because lenders are better able to forecast financial outcomes due to experiences with many borrowers (Bond, Musto and Yilmaz, 2009), or because of borrowers’ poor financial literacy (Lusardi and Tufano, 2009). In these cases, individuals will borrow even if it makes them worse off in the end.

²⁰ Data are from 1) the Federal Reserve website, Flow of Funds Accounts, Table B.100, line 32 and 2) Bricker, Jesse, Arthur B. Kennickell, Kevin B. Moore, and John Sabelhaus “Changes in U.S. Family Finances from 2007 to 2010: Evidence from the Survey of Consumer Finances.” *Federal Reserve Bulletin*, vol. 98, no 2, (February 2012), pp. 1-80.

In this paper, I ask the question “How do the effects of consumer credit access on household well-being vary with the economic state of the world?” I study the effect of access to one specific form of credit: payday lending, the market for small-value, short-term loans taken at an annual percentage rate of around 400 percent. Payday lending’s effect on household well-being has been particularly controversial. Proponents of payday lending maintain that it is an important backstop for families facing emergencies that lack access to other credit options (Andersen, 2011). Opponents of payday lending, however, charge that lenders trap poorly informed individuals in a cycle of repeated borrowing at usurious interest rates and exacerbate financial distress (Parrish and King, 2009).

I study the effects of payday lending on material well-being specifically, using data on household spending from the Consumer Expenditure Survey (CE). Consumption is a natural outcome to study with respect to credit access because in most theoretical models, households derive utility from spending and credit access affects utility through a spending channel. In addition, household spending is a better proxy of material well-being than household income from a theoretical perspective and is a common measure of material well-being in the economics literature (Meyer and Sullivan, 2004).

The payday lending market is a particularly suitable laboratory in which to evaluate the effects of credit on well-being for two reasons. First, the arguments for and against payday lending tend to mirror the theoretical arguments regarding effects of consumer credit more broadly. And empirical work to date has far from resolved the argument. Authors have found highly mixed results of payday lending on household financial conditions and other measures of well-being. On the negative side, authors have found that payday borrowing results in households reporting difficulty paying their rent, mortgage and other bills (Melzer, 2011), that it increases personal bankruptcy filing rates (Skiba and Tobacman, 2011), and that it leads to declining job performance and eligibility to re-enlist in the Air Force (Carrell and Zinman, 2008). On the positive side, authors have found that access to payday loans mitigates foreclosures following natural disasters (Morse,

2011), that banning payday lending results in more bounced checks and complaints against debt collectors (Morgan, Strain and Seblani, 2012), and that capping payday loan interest rates leads to households reporting a decline in overall financial conditions (Zinman, 2010). Bhutta (2014) finds little evidence that payday lending has any effect on household financial conditions on average. He finds no effect of payday access on credit scores, credit delinquencies, or the likelihood of overdrawing credit lines.

The second reason payday lending is a suitable laboratory is that variation in access to payday lending by geography and over time lends itself to identifying an effect of payday credit particularly well. In general, it is difficult to isolate the effect of credit access on household outcomes. Household credit and spending choices are determined simultaneously and are both likely correlated with unobserved household characteristics, leading to issues of simultaneity bias and omitted variable bias in regression analysis. In addition, access to credit is not randomly assigned. Regulators and credit providers both play a role in determining household access to credit. State regulatory actions may be confounded with other economic factors that can influence household spending. And in the payday market particularly, lenders likely make location decisions based on the characteristics of potential borrowers with the goal of maximizing profitability.

I address these challenges by following Melzer's (2011) novel identification strategy, which compares the spending patterns of two types of households that live in states banning payday lending: 1) households who live close to the border of payday-allowing bordering state and hence have access to payday loans, and 2) households that live far from the border of a payday-allowing state and hence do not have access to payday loans. This strategy ameliorates the endogeneity concerns associated with studies that use state-level changes in payday loan availability to identify the effects of lending.

I conduct two main tests. First, I analyze how payday lending affects household spending overall, in the normal state of the world. I use confidential data on the census tract of each household in the CE survey to calculate the distance of households in states prohibiting payday lending to

states allowing payday lending. I look for effects on nondurable and durable goods spending broadly as well as spending on specific items such as housing, food, and entertainment.

It is not a given that I should see any spending effects of the payday loan market overall since these loans have to be repaid and theory suggests that credit access helps households smooth consumption, not change consumption patterns. However, there are several reasons I may see an effect overall. First, if payday lending itself increases economic hardship as opponents claim and some work finds (Melzer, 2011; Skiba and Tobacman, 2011), I would expect to see that payday loan access results in overall spending declines reflecting such financial distress. Second, if the typical payday loan borrower has present-biased preferences that cause severe self-control problems, I would expect that easy access to extra cash may exacerbate over-consumption.²¹ In this case, I may observe households spending more on luxury goods and services than they would otherwise. While studying the spending effects of payday lending is not a direct test of preferences by any means, observing increases in luxury good spending for households may be indicative of self-control problems.

The second test I carry out is to directly study whether payday loan access helps families smooth consumption during periods of temporary financial distress in a “bad” state of the world. I use extreme weather events such as hurricanes and blizzards as an exogenous, negative shock to households. I test whether households with payday loan access have higher spending after the event than those without payday loan access. Severe weather events are strictly exogenous with respect to spending and payday loan access and they also plausibly represent periods of temporary financial distress. Severe storms can cause damage to one’s home or car, for example, requiring unexpected outlays for repairs. Or bad weather can close one’s workplace, causing a temporary drop in income for hourly workers. This analysis is similar to Morse (2011),

²¹ Payday borrowers are often associated with having present-biased preferences in the literature. The frequent rollover of payday loans despite the high interest rates is consistent with non-standard preferences (Melzer, 2011). Estimating a dynamic programming model of consumption, saving, borrowing and default, Skiba and Tobacman (2008) find default patterns among payday loan users to be most consistent with partially-naive quasi-hyperbolic discounting specifically. And Parsons and Van Wesepe (2012) examine the welfare effects of payday credit using a model where agents are paid at regular intervals and are present-biased sophisticates.

but I use a broader set of extreme weather events occurring over a wider geographic area and time horizon. In addition, Morse's work studies the effect of payday lending on foreclosures while my work studies household consumption, allowing for a direct test of consumption smoothing.

My findings show that the effects of payday credit on household spending are heterogeneous and state dependent. First, I show that granting households access to payday lending *reduces* household material well-being on average, in a normal state of the world. Payday loan access reduces aggregate reported household spending, with the majority of the spending reductions occurring in shelter and food expenditures. I find that households with access to payday lending report lower total expenditures, and that this effect is distributed in both nondurable and durable spending. These results are concentrated in households with a greater propensity to be payday borrowers—those with income between \$15,000 and \$50,000. In terms of the concentration of spending reductions, I find that the spending reduction is concentrated in spending on shelter (including rental payments as well as mortgage payments) and food (food at home and food away from home) particularly. These results are consistent with loan access causing households overall financial distress as critics contend. They are particularly consistent with Melzer's (2011) result that households with payday loan access report having difficulty paying their rent, mortgage and other bills. I find only weak evidence that payday loan access results in an increase in spending on luxury or so-called temptation goods; I see some evidence that households in the \$15,000 to \$50,000 income range increase the level of spending on alcohol and tobacco products but I see no change in spending on entertainment and I see a reduction in spending on apparel.

My second main finding shows that in a bad state of the world—following a temporary period of financial distress—access to payday lending *increases* material well-being for the average household. For households *without* payday loan access, an extreme weather event lowers spending on nondurables (defined broadly) by \$22 on average in the month of the event. For those *with* payday loan access, however, spending is \$35 higher after the shock than for those without access. In particular, I find that payday loan access mitigates declines on food at home

consumption, shelter spending, mortgage payments, and home repairs. Households *without* payday loan access spend \$31 and \$18 less on shelter and home repairs in the month of an extreme weather event than in a non-event month. Households *with* payday loan access spend \$30 and \$36 more than households without access after the weather event. These results provide a direct test showing following periods of financial distress, payday loan access smooths consumption.

My work contributes to the empirical literature on payday lending by 1) highlighting the heterogeneous, state-dependent nature of the effects of this market on household well-being and by 2) reconciling some of the conflicting evidence to date on the welfare effects of payday lending. As noted above, authors have found highly mixed results on the effects of payday loan access on household well-being. To date, it has been difficult to reconcile these mixed results in the literature, in large part due to the apples-and-oranges nature of the datasets and methodologies used in the various analyses; the analyses were often simply not comparable. Most studies find evidence of either positive or negative effects of payday lending on well-being. As Melzer (2011) writes, for example: “I find no evidence that payday loans alleviate economic hardship.” It is difficult to know if the conflicting findings are due to bias resulting from methodological issues or if access to the payday loan market did have such heterogeneous effects. My work shows that indeed, the effects of payday loans on household well-being are heterogeneous and depend on whether the household is currently undergoing a period of temporary distress or not. In bad states of the world, I find that payday lending helps smooth consumption and improves material well-being. In normal states of the world, however, it worsens material well-being for households.

My work should also be of interest to policymakers considering actions targeted at payday lenders. The payday market remains the subject of much public policy attention in the United States. Since 1999, 19 states have changed the legality of payday lending, with 11 allowing the practice and 8 prohibiting it; a total of 14 states ban payday lending at present (Morgan, Strain

and Seblani, 2012). In 2007, Congress responded to criticism that payday lenders target service members by passing legislation that caps interest rates on loans to military personnel, effectively banning payday lending to these individuals. In 2012, the Consumer Financial Protection Bureau (CFPB) held hearings on payday lending to help gauge the potential role for additional federal supervision of the market (CFPB, 2012). The CFPB has since included payday lenders as institutions under their supervision and has taken several enforcement actions against payday lenders for deceptive practices (CFPB, 2014). My results suggest that regulators' concerns about payday lending worsening household financial conditions overall are valid. However, my results showing that payday lending does help households smooth consumption after temporary periods of financial distress points to the need for continued access to emergency credit for credit-constrained households. Eliminating access to the payday loan market entirely could worsen well-being for households in distress.

The remainder of the paper proceeds as follows. Section 2.2 gives an overview of the payday loan market. Section 2.3 presents the empirical methodology used for the analyses of the overall effect of payday loan access and the effect of payday loan access after temporary periods of financial distress. Section 2.4 describes the data used. Section 2.5 discusses the results and I conclude in Section 2.6.

2.2 Overview of the Payday Loan Market

Payday lending is the practice of using a post-dated check or electronic checking account information as collateral for a short-term, low-value, high interest rate loan. To qualify, borrowers need personal identification, a valid checking account, and proof of steady income from a job or government benefits, such as Social Security or disability payments.

The typical loan size ranges from \$100 to \$500 over a term of two weeks, the usual time span between paydays, and the majority of loans are for \$300 or less (Elliehausen 2009). Payday lenders usually charge an average of \$10 to \$20 per \$100 borrowed, which implies an interest

rate of about 260% to 520% APR. Of new payday loans, 36% are repaid at the end of the initial loan term and about another 20% are renewed once or twice. A considerable fraction of new loans are renewed numerous times, however. Twenty-two percent are renewed six or more times and over 10% of new loans are renewed ten or more times. Most borrowers take out just one series of loans in a year (48%), but 26% of borrowers take out two series of loans, 15% take out three series of loans, and 11% take out four or more series a year (CFPB, 2014).

In 2010, about 12 million individuals were estimated to have taken out a payday loan (PEW, 2012). Loan volume for store-front locations was estimated at \$29.3 billion that year, with revenue of \$4.7 billion. Online payday loan volume, which has been growing rapidly, was estimated at \$10.8 billion with \$2.7 billion in fees (Stephen's Inc., 2012). Looking at demographics of borrowers, they are more likely to be female, single-parents, African American, and have a high-school degree or some college education than the general population (Bourke, Horowitz and Roche, 2012). Since one generally needs a valid bank account and pay stub as proof of employment to qualify for a loan, payday borrowers are not in the poorest population cohort; still, the typical borrower is part of a lower-than-average income household. Twenty-five percent of payday borrowers report income of less than \$15,000, while 56% have income between \$15,000 and \$50,000 and 16% report income greater than \$50,000 (Bourke, Horowitz and Roche 2012; note, the breakdown does not sum to 100% because some households do not report income).

Payday loan borrowers also tend to have limited liquid assets and be credit constrained. About 55% of borrowers reported not having savings or reserve funds in 2007. At the time of taking out their most recent payday loan, about 45% reported not having a credit card and 22% reported that they would have exceeded their credit limit if they had used a credit card. Twenty-eight percent said they could have borrowed from a friend or relative, and 17% said they could have used savings (Elliehausen, 2009).

In survey evidence for why households take out payday loans, 69% of borrowers reported using their first loan for "recurring expenses:" 53% for regular expenses like utilities, car payments or

credit cards, 10% for rent or mortgage payments, and 5% for food (Bourke, Horowitz and Roche 2012; note, the breakdown does not add to 69% due to rounding). Sixteen percent of payday borrowers in the survey report using the loan for an “unexpected emergency/expense” while 8% report using the loan for “something special,” and 7% report “other” or “don’t know.”

2.3 Empirical Methodology

2.3.1 Overall Effect of Payday Loan Access

To test the overall effect of payday loan access on household spending, I follow Melzer (2011) and use a strategy that relies on variation in access to payday lending geographically and over time. Many studies rely on state-level variation in the legality of payday lending or variation in households’ proximity to a payday lender to identify an effect of lending on household outcomes (Table 1 summarizes the state law changes).²² These strategies raise concerns, however. Legislative decisions are likely to be correlated with household financial conditions or other state-level policies that may affect household welfare, which would result in the difference-in-difference analysis not identifying a causal effect of payday loan access. Lenders’ location decisions are also likely correlated with household characteristics and financial conditions, which may limit a causal analysis.

To ameliorate these endogeneity concerns, Melzer’s strategy takes advantage of variation that is independent of state-level legislative decisions or households’ proximity to particular payday lending locations. The strategy compares two types of households that live in states that ban payday lending: 1) households that live close to the border of a state that allows payday lending and hence, still have relatively easy access to the payday loan market and 2) households that live far from the border of a payday-allowing state and hence, have limited payday-loan access. Melzer provides suggestive evidence that borrowers travel across state borders to obtain payday

²² In order to preserve the confidentiality of the Consumer Expenditure Survey sampling areas, I cannot report the payday-banning states included in the sample.

loans—payday lenders have a higher propensity to locate near the borders of states that prohibit payday loans after conditioning on local observable economic conditions.

The empirical model is as follows:

$$(1) \text{Expenditure}_{ict} = \beta_1 \text{PaydayAccess}_{ct} + \beta_2 \text{Border}_c + \gamma W_{it} + \delta X_{st} + \omega Z_{nt} + \alpha_s + \alpha_t + \epsilon_{ist}$$

In this model, *i* indexes households, *c* indexes census tracts and *t* indexes the month in which a particular quarter's spending ended. Expenditure is the dollar value or the log dollar value of spending over the quarter ending in month *t*. The regression sample is limited to households in states that ban payday lending. PaydayAccess is a dummy variable that equals 1 if a household in a state that bans payday lending lives in a census tract within 25 miles of a state that allows payday lending—Melzer's cutoff for living close to a payday-allowing state. PaydayAccess equals 0 if a household lives in a state that bans payday lending but the household's census tract is farther than 25 miles from the border of a state that allows payday lending. *W* is a vector of household-level controls: housing tenure, education level of the survey's reference person, race of the survey's reference person, age of the reference person, family size, income class, and a cubic in household income (as a proxy for permanent income). *X* is a vector of state-level controls: personal income growth, the log of personal income, and the log of house prices. *Z* is a vector of county-level controls: the unemployment rate and employment growth. I include fixed effects for state and month (final month of the quarterly survey) in the model and cluster the standard errors at the county level. I estimate the model using OLS for all households in the sample as well as for households with income between \$15,000 and \$50,000 (households with the greatest propensity to be payday borrowers, as in Melzer, 2011).

2.3.2 Effect of Payday Loan Access after a Temporary Negative Shock

In order to directly test whether payday lending helps households smooth consumption following periods of temporary financial distress, I analyze whether payday loan access affects household spending following an extreme weather event. Extreme weather events are exogenous with

respect to household spending and represent plausible temporary, negative shocks to household finances. An extreme weather event could prevent an hourly employee from making it to work for several days, for example, acting as an income shock. In addition, weather could cause damage to one's home or car, requiring an unexpected outlay for repairs. This is a similar strategy used by Morse (2011), except that Morse's analysis relies on interacting the weather event with the presence of a payday lender in a household's zip code. As discussed above, defining payday loan access as proximity to the border of a payday-allowing state has the advantage of being independent from store location decisions.

To perform this analysis, I examine the interaction of access to payday lending and weather shocks. I interact *PaydayAccess* with the dummy variable *WeatherEvent* that equals 1 if any weather event that caused monetary damages occurred in the county in which a particular census tract was located. The empirical model is as follows:

$$\begin{aligned}
 (2) \text{ Expenditure}_{ict} &= \beta_1 \text{PaydayAccess}_{ct} + \beta_2 \text{WeatherEvent}_{nt} \\
 &+ \beta_3 \text{PaydayAccess} \times \text{WeatherEvent}_{cnt} + \beta_4 \text{Border}_c + \gamma W_{it} + \delta X_{st} + \omega Z_{nt} + \alpha_s \\
 &+ \alpha_t + \epsilon_{ist}
 \end{aligned}$$

The time indicator *t* now represents the month of household spending; I use monthly expenditures in this analysis to match the month of the income shock with the month of spending. *PaydayAccess* is defined as in the section above. The household-level, state-level, and county-level controls are the same as above and I also include state and month fixed effects and cluster standard errors at the county level.

In this model, the coefficient β_2 measures the spending effects of experiencing an extreme weather event in a given month when a household does not have access to payday lending. The coefficient β_3 measures the difference in spending after a weather event for households with payday loan access compared to households without payday loan access. This

coefficient will be positive if payday credit access boosts household spending during temporary, negative shocks. The total spending effect of a weather shock when a household has payday loan access is then $\beta_2 + \beta_3$. The spending effect of allowing payday lending when no weather shock has occurred is measured by the coefficient β_1 .

2.4 Data

2.4.1 Consumer Expenditure Data

The main outcome variables of interest in this analysis are categories of household spending including broad measures of spending (overall spending on durable goods and nondurable goods) as well as more narrow categories (e.g., food, shelter, utilities and health care). I use data from the Consumer Expenditure Survey (CE), Interview Survey, a nationally representative survey of spending that is published by the Bureau of Labor Statistics (BLS). In the CE survey, households are interviewed for five consecutive quarters on their spending over the previous three months.²³ In addition to including highly detailed data on household spending, the survey also includes detailed data on household demographics and data on household balance sheets. There are about 7,000 households surveyed a quarter, for a total of about 28,000 surveys collected a year and there are a total of 91 geographic sampling areas across the country.

The geographic information available in the public-use Consumer Expenditure (CE) survey data files are limited to state and MSA-level indicators, which are only available for a subset of households. In order to construct the measure of a CE household's distance to the closest state that allows payday lending, I use confidential data on each household's census tract location accessed at the BLS headquarters.

I study four aggregate measures of expenditures as well as a number of specific spending categories. The aggregate measures that I study are 1) total household expenditures, 2) a broad

²³ Note, a "consumer unit", which is defined an independent financial entity within a household, is the unit of observation in the survey. I will use the term "household" interchangeably with consumer unit.

measure of nondurable expenditures, 3) a narrow measure of nondurable expenditure categories (following Lusardi, 1996), and total durable goods. The specific expenditure categories I use follow from the major breakdown of goods as in Kearney (2004). I deflate expenditures to constant 2010 dollars using the consumer price index for all urban consumers (CPI-U, not seasonally adjusted). For analysis of the overall effect of payday loan access on household spending, I use data at the quarterly spending level.

To construct the sample, I follow the literature in limiting the sample to exclude households living in student housing, those that report an age of less than 21 or greater than 85, those that incompletely report income, those that report age changing by more than one between quarters, or those that report the number of children changing by more than 3 between quarters. I provide a detailed description of how the spending variables, household credit variables, and data sample were constructed in Appendix 4. I use a data sample from 1998 to 2010 as the payday lending market started developing in the 1990s and the first payday loan access law change was in 1999. I end the sample in 2010 in order to limit confounding effects of the online payday lending market, which has been growing over time (Bourke, Horowitz and Roche 2012). Since households in any state may access payday loans online, the growth of this market confounds the geographic variation used to identify the effects of payday loan access in this paper.

Table 2 presents summary statistics for the expenditure categories that I analyze in this study—quarterly average spending levels and standard deviations, indexed to 2010 dollars using the CPI-U. Column 1 shows households that do not have access to payday lending and column 2 shows households that have access to payday spending (about 70 percent of the qualified household). Average spending for both groups totals around \$11,000 a quarter with spending on durable goods making up about two-thirds of total spending. Nondurable spending defined broadly totals about \$3,750 a quarter while nondurables spending defined narrowly totals about \$2,750. The largest individual categories of spending are shelter (\$2,500), transportation (\$2,000) and food at home (\$1,100). Notably, while there is no statistical differences in the aggregate

spending levels of each group, there are larger differences in the breakdown of spending by detailed category. Households without payday loan access spend more on housing, food, and apparel expenditures, while households with payday loan access spend more on health care and entertainment.

I present summary statistics for household demographics of households with and without payday loan access in Table 3. There is no statistical difference between these households in terms of income, marital status, or education levels. Households without payday access are more likely to be homeowners and the family size is slightly larger in households with payday loan access (2.54 versus 2.51). The share of Caucasian households does not differ between the two samples, but the rest of the racial composition does; households with access to payday lending are more likely to be Hispanic or Asian and less likely to be African American.

2.4.2 Weather Event Data

To test whether payday lending improves material well-being in the face of a negative shock to household financial conditions, I use data on extreme weather events from the University of South Carolina's Sheldus Hazard Database. This database compiles county-level information on dollar losses and fatalities from 18 types of events including hurricanes, thunder storms, floods, and blizzards. By using data on household location, I can more precisely match extreme weather events to the households most likely to have been affected by these weather events. As discussed above, in order to more precisely match the timing of weather events to the timing of household spending, I use monthly spending data in the CE files for this analysis.

I present summary statistics for the weather event dataset in Table 4. In order to preserve confidentiality of the CE sampling areas, the information I present is limited but shows that extreme weather events occur frequently for households in the CE sample studied here and that the economic magnitude of these events is economically meaningful. Of the total number of monthly household spending observations in the sample (192,000), weather caused property

damage in a household's county in about a third of those months (67,000). These weather events affect a considerable number of households with payday loan access; among these households, there were 22,000 monthly household observations in which weather damage was recorded in a household's county. In any month with damage, the average property damage recorded for a county was about \$1.4 million. The weather events with the greatest frequency of occurring in the total sample are storm events (25,782), wind events (23,094), wind-related winter weather (9,460) and flooding (8,518). Multiple weather events in a given month are a frequent occurrence.

2.5 Results

2.5.1 Results: Overall Effect of Payday Loan Access

I first investigate the overall effect of payday loan access on aggregate household expenditures. Table 5 shows the estimated coefficient on *PaydayAccess* from the regression specification in equation (1); the table shows results for four measures of aggregate spending: total expenditures, nondurable expenditures defined broadly, nondurable expenditures defined narrowly, and durable good expenditures. I present results for all households in the sample as well as for households with incomes between \$15,000 and \$50,000—the income range in which the majority of payday loan borrowers fall (following Melzer (2011)). I present results for specifications with household expenditures defined both in levels and the natural logarithm of expenditures. The coefficient in the levels specification can be interpreted as the dollar change in quarterly household spending resulting from access to the payday loan market. The coefficient in the log-linear specification can be interpreted as the percentage change in quarterly household spending resulting from access to the payday loan market. Utilizing a log-linear specification has the advantage of mitigating the effects of any outliers in the regression; for this reason, the log-linear specification may be preferred to the levels specification.

The results show households with payday loan access have lower household spending on average, across aggregate spending categories. The estimated coefficient on *PaydayAccess* is

negative in each regression, indicating that payday access reduces household expenditures on aggregate expenditures—nondurable expenditures as well as durable expenditures. For all households on the sample, I find that payday access results in a 5.5 percent reduction in total household spending on average. The corresponding dollar value reduction is about \$600 a quarter, although this estimate is not statistically significant. The results indicate that payday loan access reduces nondurables spending using both the narrow and broad definitions of nondurables spending. Nondurable spending defined narrowly falls by about \$220 a quarter (6.3 percent), while nondurable spending defined broadly falls by about \$310 a quarter (6.3 percent); the estimated effect of payday loan access is significant in both the levels and log-linear specification. As there are 1.7 adults per household, this corresponds to a monthly spending reduction of about \$40 and \$60 a month, respectively. I find a reduction in durables spending as well (5.3%), although again the reduction is only statistically significant for the log specification. I see similar results when limiting the data sample to households in the \$15,000 to \$50,000 income class. I see that households in this income range also report lower household expenditures across aggregate spending categories. In this set of regressions, however, the effect of payday loan access on household spending is statistically significant more often in the levels specification. The effect on overall expenditures is now significant when measured in levels as well as in logs; the coefficient can be interpreted as households with payday loan access reporting \$575 lower total expenditures (\$112 per adult, per month).

The relatively large magnitude of the regression coefficient estimates raises the question of whether these magnitudes are plausible. It is likely that loan fees for payday loans are underreported in the CE and that the reduction in aggregate expenditures reflects a reduction in expenditures excluding loan charges. While banking fees and finance fees are reported in the quarterly CE survey, households are known to underreport expenditures for so-called “sin” goods and services (gambling, alcohol and tobacco for example), of which payday loan fees may be included. The average payday loan has a \$20 fee per \$100 of loans spent and since the typical loan is around \$300 or less, that implies a fee of about \$60 per loan. A \$125 spending reduction

per adult, per month would suggest that two loans are being taken out per person in the survey on average each month. While a large fraction of payday loans are rolled over for at least one additional period and payday borrowers report taking a number of loans through the month, this is likely an implausibly large magnitude. Below I investigate other explanations for the spending reduction than the reduction solely being due to a payday loan charges not being included in reported household spending.

Next I examine how the spending reductions are split between the detailed expenditure categories. Table 6 shows the coefficient on *PaydayAccess* from empirical specification (1), with each row representing a separate regression coefficient on the listed expenditure category as the dependent variable. Columns 1 and 2 in the table show estimates from a log-linear and linear regression specification, respectively, for all households in the sample. Columns 3 and 4 show corresponding estimates for households in the \$15,000 to \$50,000 income category. I find that households with payday loan access report the largest reductions in spending on shelter and on food. I see that households with payday loan access on average spend \$570 less a quarter on shelter (a category that includes broad expenditures on both owned dwellings and rented dwellings). Shelter expenditures only include spending on mortgage interest, not mortgage principle. The mortgage category reported in the table shows total mortgage payment spending (principle and interest) and the results show that households with payday loan access spend about \$250 less a quarter on mortgage expenditures. Households with payday loan access spend about \$150 less in rent payments per quarter.

The reductions in spending on food resulting from payday loan access are also substantial. These households spend \$87 and \$88 less a quarter on food at home and food away from home, respectively, than households without payday loan access. The coefficient estimates are significant for these expenditure categories in both the level and the log-linear regression specifications, for all households and for households in the \$15,000 and \$50,000 income category. The other notable category of spending declines is in apparel; households spend \$72

less on apparel a quarter and the reductions in apparel spending are significant across all 4 specifications reported in the table. I see some small reduction in health care spending for households with payday loan access, although only in the log-linear specification for all households.

These results are in line with Melzer's (2011) findings that access to payday loan credit overall causes households to report having more difficulty paying the rent, the mortgage, and medical bills. They also accord with his conclusions that for low-income households, payday loan fees result in households having fewer funds to spend on other bills.

One channel for payday loan access affecting other categories of household spending is if loan fees result in households having fewer funds available for other expenditures. Another reason that payday loan access could affect household spending, however, is if the typical payday loan borrower has present-biased preferences that cause severe self-control problems. In this case, easy access to extra cash may exacerbate over-consumption, causing households to spend more on luxury goods and services than they would otherwise. I investigate this hypothesis by looking at whether payday loan access causes any change in spending on in so-called temptation goods (as in Bertrand and Morse, 2009), particularly spending on alcohol, tobacco, and entertainment. I only find weak evidence to support this hypothesis. I find that households with payday access in the \$15,000 to \$50,000 income category report a \$45 increase a month in spending on alcohol and tobacco products, and this increase is significant at the 1 percent level. It is not significant in the other specifications, however. I also see no significant increase in entertainment in spending overall.

Finally, I also find a sizeable increase in transportation spending for households with payday loan access (\$194 or about an 8 percent increase). This result raises the question of whether *PaydayAccess* is correlated with other commuting-related expenses that may be affecting the other spending results as well (perhaps explaining why the magnitude of the effects is so large). I have further work to do to investigate this possibility.

2.5.2 Results: Effect of Payday Loan Access following a Temporary Negative Shock

Next, I investigate the whether access to the payday loan market affects spending following periods of temporary financial distress, represented by an extreme weather event occurring in the month. Using the extreme weather events as a natural experiment, this analysis provides a direct test of whether credit access helps household smooth spending around negative shocks. First I study the effects on aggregate household spending, using the four measures studied above. Table 7 shows results from empirical specification (2), which interacts the effects of payday loan access and a weather event occurring in a given month. Each column represents one regression of the dependent variable named at the top of the column on the explanatory variables as well as the control variables described above. Panel A of Table 7 shows results for the specification with the dependent variables in levels and Panel B shows results for the natural logarithm of the dependent variable.

I find evidence that payday lending does play a valuable consumption smoothing role for households facing temporary periods of financial distress; households with payday loan access spend more on nondurables after temporary, negative financial shocks than those without payday loan access. For households without payday loan access, an extreme weather event lowers monthly spending on nondurables defined broadly by \$22 on average and on nondurables defined narrowly by \$15 on average. For those with payday loan access, however, monthly spending is \$35 higher and \$30 higher on broad and narrow nondurables, respectively, than for those without access after the weather shock. I see a similar result in the log-linear specification. An extreme weather event reduces reported household spending on both broad and narrow nondurables by 1.4 percent and 1.5 percent, respectively for households without payday access. Household with payday loan access, however, report 2.8 percent and 2.6 percent higher spending than households without payday loan access following the weather event. I do not see an effect on total expenditures in either specification, however, because there is no statistically significant effect on durable good spending.

Looking at the effect of payday loan access on specific spending categories following a weather event (Table 8), I find a similar pattern as above for expenditures on several specific categories—food at home, shelter spending, mortgage payments, and home repairs. As in Table 7, Panel A shows a specification with the expenditure measured in levels and Panel B shows a specification with the expenditure measured as a natural logarithm. I show results for the categories with statistically significant results. The results for food expenditures at home are the most robust across specifications. Extreme weather events result in a reduction of \$7 (1.5 percent) in monthly spending on food at home for households without payday loan access. For those with payday access, spending on food at home is \$12 (2.9 percent) higher after the weather event than for those without payday loan access.

Shelter and home repairs are a second category in which I see statistically significant effects of payday loan access following an extreme weather event. For households without access to payday lending, monthly expenditures on shelter overall and on mortgage payments are \$31 and \$11 lower, respectively. For those with payday access, spending is \$30 and \$19 higher after the weather event than for those without. Home repair expenditures are \$18 lower following a weather event for households without payday loan access. Payday loan access more than mitigates that decline in home repairs; households with access spend \$36 more on home repairs after the weather event than those without access. These results provide a clean test that following periods of financial distress, payday loan access helps households smooth consumption. My result for mortgage payments are in line with Morse's (2011) results that show payday lending mitigates the increase in foreclosures that occurs following natural disasters in California. I build on Morse's work by showing a direct consumption smoothing mechanism that mitigating financial distress. In addition, I show that the consumption effect is broader than in mortgage payments alone.

2.6 Conclusion

In this paper, I investigate whether households benefit from increased access to payday credit—a market that has grown rapidly since the late 1990s and that has come under regulatory scrutiny for the high fees charged per loan transaction. I study the effects of payday loan access on household material well-being for households in two states of the world: 1) the average effect in a “normal” state of the world and 2) the effect of access in a “bad” state of the world (households that have recently experienced a temporary, negative shock to household finances). I show that the effect on material well-being is state dependent. Under normal conditions, payday loan access reduces average household spending on non-loan expenditures substantially, particularly expenditures on rent, mortgage payments and food. After temporary periods of financial distress (an extreme weather event), however, payday loan access mitigates the spending declines that occur for households that experience the shock but don’t have access to payday credit; loan access helps households smooth consumption over the shock.

These results provide empirical evidence on the heterogeneous nature consumer credit’s on household well-being; the effects vary even *within* the market for one specific credit product. The finding that payday loan access results in household spending declines overall is consistent with evidence in the literature to date that payday lending is indeed associated with increased economic hardship for households overall. In distressed conditions, however, payday lending does appear to aid households facing emergencies, helping households keep food on the table and pay the mortgage.

Figures and Tables

Table 1: Payday Loan Laws by State

Always Banned		Always Legal		Banned	Legalized
CT	CA	KY	OH	AR (Dec. 07)	AL (Jun. 03)
ME	CO	LA	SC	DC (Nov. 07)	AK (Jun. 04)
MA	DE	MN	SD	GA (May 04)	AZ (Apr. 00)
NJ	FL	MS	TN	MD (Jun. 00)	AR (Apr. 99)
NY	ID	MO	TX	NC (Dec. 05)	HI (Jul. 99)
VT	IL	MT	UT	OR (Jul. 07)	MI (Nov. 05)
	IN	NE	WA	PA (Nov. 07)	NH (Jan. 00)
	IA	NV	WI	WV (Jun.06)	ND (Apr. 01)
	KS	NM	WY		OK (Sep. 03)
					RI (Jul. 01)
					VA (Apr. 02)

Source: Morgan, Strain, and Seblani, 2012

Table 2: Summary Statistics, Expenditure Categories

	Payday Access = 0		Payday Access = 1		(P-value difference)
	Mean	SD	Mean	SD	
Total Expenditures	11,069	10,527	10,959	9,738	0.20
Nondurables: Narrow	2,758	3,262	2,733	2,320	0.27
Nondurables: Broad	3,750	3,854	3,739	3,076	0.73
Durable Goods	7,320	7,820	7,220	7,794	0.14
Food at home	1,149	759	1,132	742	0.01
Food away from home	471	900	454	933	0.03
Shelter	2,579	2,944	2,317	2,519	0.00
Rent Payments	723	1,290	543	1,105	0.00
Mortgage Payments	1,062	2,085	1,187	2,146	0.00
Utilities	844	607	869	541	0.00
Household Operations	529	1,667	517	1,519	0.37
Health Care	596	934	653	918	0.00
Education	254	1,718	255	1,752	0.96
Alcohol and tobacco	172	325	176	326	0.15
Apparel	360	666	318	972	0.00
Entertainment	526	1,151	551	1,814	0.07
Transportation	1,796	3,861	1,827	3,950	0.35
Sample size:	44,332		19,276		

Table 3: Summary Statistics: Demographic Variables

	Payday Access = 0		Payday Access = 1		(P-Value of Difference)
	Mean	Std. Dev.	Mean	Std. Dev.	
Income	51.10	61.91	51.09	59.44	0.99
Married	0.54	0.50	0.54	0.50	0.31
Homeowner	0.65	0.48	0.71	0.46	0.00
Family Size	2.56	1.47	2.51	1.41	0.00
Age	50.39	15.84	50.25	15.63	0.32
Race					
White	0.79	0.41	0.79	0.40	0.83
Black	0.16	0.36	0.16	0.37	0.11
Asian	0.04	0.19	0.03	0.16	0.00
Hispanic	0.10	0.30	0.05	0.22	0.00
Other	0.01	0.10	0.02	0.13	0.00
Education					
Below High School	0.15	0.36	0.15	0.36	0.20
High School	0.28	0.45	0.29	0.45	0.27
Some College	0.26	0.44	0.26	0.44	0.81
Bachelors or higher	0.31	0.46	0.30	0.46	0.77
Sample size:	44,332		19,276		

Table 4: Summary Statistics: Weather Events

Obs. in data sample:	192,329
Obs. with a weather event in the county:	
Any	66,748
Flooding	8,518
All Storm Events	25,782
Wind	23,094
Wind/Winter weather	9,460
Obs. with payday loan access and any weather event in the county:	22,178
Mean county property damage in a month with a weather event:	\$1,366,424

Table 5: Effect of Payday Loan Access on Household Expenditures

	All Income		Income 15-50K	
	Ln (1)	Level (2)	Ln (3)	Level (4)
Dependent Variable:				
Total Expenditures	-0.0556** [0.0257]	-599.6 [366.4]	-0.0484* [0.0263]	-575.3* [294.3]
Nondurables: Narrow	-0.0626** [0.0301]	-218.9** [103.1]	-0.0512 [0.0359]	-162.2* [90.50]
Nondurables: Broad	-0.0629** [0.0276]	-313.5** [129.3]	-0.0441 [0.0312]	-260.1** [114.3]
Durable Goods	-0.0530* [0.0273]	-286.1 [252.3]	-0.0531* [0.0278]	-315.2 [204.6]
Obs.	63,605	63,605	21,028	21,028

This table presents results from empirical specification (1), regressions of quarterly expenditure categories on *PaydayAccess*, household-level controls (housing tenure, education level, race, age, family size, income class, and a cubic in household income), state-level controls (personal income growth, the log of personal income, and the log of house prices), county-level controls (the unemployment rate and employment growth) and state and year fixed effects. Each cell reports estimates for a separate regression using the dependant variables listed by row. Standard errors are presented in brackets below the coefficient estimates and are clustered at the county level. The sample period is 1998 to 2010. ***, **, and * indicate 1 percent, 5 percent and 10 percent significance, respectively.

Table 6: Effect of Payday Loan Access on Household Expenditures

	All Income		Income 15-50K	
	Ln	Level	Ln	Level
	(1)	(2)	(3)	(4)
Dependent Variables:				
Shelter	-0.188*** [0.0559]	-571.4*** [170.5]	-0.202*** [0.0624]	-454.4*** [138.9]
Rent Payments	-0.140** [0.0651]	-149.2** [59.98]	-0.164** [0.0758]	-194.5*** [71.39]
Mortgage Payments	-0.202*** [0.0595]	-257.6*** [87.99]	-0.287*** [0.0814]	-156.2** [60.58]
Food At Home	-0.0698** [0.0316]	-86.92** [38.71]	-0.0844** [0.0428]	-115.4** [46.88]
Food Away From Home	-0.161*** [0.0510]	-87.52*** [30.11]	-0.169** [0.0661]	-71.96** [31.23]
Alcohol and Tobacco	-0.036 [0.0395]	15.01 [10.75]	0.0721 [0.0625]	43.96*** [14.64]
Utilities	0.0285 [0.0275]	-9.389 [22.06]	0.0598** [0.0284]	25.46 [21.30]
Health Care	-0.0606** [0.0294]	-29.65 [24.78]	-0.0452 [0.0444]	-39.45 [33.16]
Transportation	0.0818* [0.0424]	194.3** [88.32]	0.0926* [0.0539]	144.6 [111.6]
Education	-0.182 [0.115]	-2.62 [37.90]	0.0913 [0.156]	26.72 [31.91]
Apparel	-0.144*** [0.0445]	-72.46*** [22.79]	-0.115** [0.0572]	-67.52*** [21.70]
Entertainment	0.0153 [0.0285]	28.16 [28.60]	0.0133 [0.0341]	0.449 [20.82]
No. Households	63,605	63,605	21,028	21,028

This table presents results from empirical specification (1), regressions of quarterly expenditure categories on *PaydayAccess*, household-level controls (housing tenure, education level, race, age, family size, income class, and a cubic in household income), state-level controls (personal income growth, the log of personal income, and the log of house prices), county-level controls (the unemployment rate and employment growth) and state and year fixed effects. Each cell reports estimates for a separate regression using the dependant variables listed by row. Standard errors are presented in brackets below the coefficient estimates and are clustered at the county level. The sample period is 1998 to 2010. ***, **, and * indicate 1 percent, 5 percent and 10 percent significance, respectively.

Table 7: Effect of Payday Loan Access on Expenditures After Extreme Weather Events

Panel A: Level Specification

	Dependent Variable:			
	Total Expenditures	Nondurables: Narrow	Nondurables: Broad	Durables
WeatherEvent	-51.25 [31.62]	-15.37* [7.931]	-22.04* [11.68]	-29.21 [23.48]
WeatherEventXPaydayAccess	84.96 [53.15]	30.15** [14.34]	34.90* [20.26]	50.06 [39.56]
PaydayAccess	-88.39 [100.5]	-46.64 [28.68]	-67.02* [35.75]	-21.37 [71.08]
Obs.	192,148	191,955	192,012	192,100
R-squared	0.466	0.426	0.41	0.411

Panel B: Ln Specification

	Dependent Variable:			
	Total Expenditures	Nondurables: Narrow	Nondurables: Broad	Durables
WeatherEvent	-0.00992 [0.00727]	-0.0140** [0.00709]	-0.0145** [0.00729]	-0.00449 [0.00789]
WeatherEventXPaydayAccess	0.0151 [0.0130]	0.0281** [0.0122]	0.0255* [0.0132]	0.000426 [0.0153]
PaydayAccess	-0.03 [0.0219]	-0.0376 [0.0249]	-0.0415* [0.0230]	-0.019 [0.0244]
Obs.	192,148	191,955	192,012	192,100
R-squared	0.466	0.426	0.41	0.411

This table presents results from empirical specification (2). WeatherEvent is a dummy variables equal to 1 if a household lives in a county that experienced a weather event in a month. Regressions include household-level controls (housing tenure, education level, race, age, family size, income class, and a cubic in household income), state-level controls (personal income growth, the log of personal income, and the log of house prices), county-level controls (the unemployment rate and employment growth) and state and year fixed effects. Standard errors are presented in brackets below the coefficient estimates and are clustered at the county level. The sample period is 1998 to 2010. ***, **, and * indicate 1 percent, 5 percent and 10 percent significance, respectively.

Table 8: Effect of Payday Loan Access on Detailed Expenditures After Extreme Weather Events

Panel A: Level Specification

	Dependent Variable:									
	Food At Home	Food Away from Home	Shelter	Rent	Mortgage Payments	Utilities	Health Care	Apparel	Transportation	Home Repairs
WeatherEvent	-7.183** [2.805]	-0.626 [3.183]	-31.16*** [9.639]	-6.932** [3.242]	-11.18* [5.712]	3.800* [2.273]	-0.656 [3.919]	-5.793** [2.773]	-3.591 [15.30]	-18.10* [9.691]
WeatherEventXPaydayAccess	12.11** [5.573]	5.546 [4.860]	30.16* [17.09]	5.38 [4.927]	18.72** [8.980]	-3.303 [4.088]	5.821 [6.638]	-1.626 [6.021]	17.97 [23.38]	35.69* [18.50]
PaydayAccess	-15.15 [10.74]	-21.15*** [7.312]	-115.4*** [43.94]	-22.07 [14.65]	-59.84** [25.58]	2.681 [5.765]	-10.39 [7.620]	-12.88* [7.533]	45.80* [26.24]	3.566 [23.66]
No. Obs	191,003	147,242	189,543	62,771	73,276	187,429	143,062	116,778	178,496	30,102
R-squared	0.373	0.244	0.484	0.381	0.247	0.323	0.164	0.141	0.256	0.084

Panel B: Ln Specification

	Dependent Variable :									
	Food									
	Food at home	away from home	Shelter	Rent	Mortgage Payments	Utilities	Health Care	Apparel	Transportation	Home Repairs
WeatherEvent	-0.0145** [0.00659]	-0.0230** [0.0113]	-0.00493 [0.0106]	0.00282 [0.0106]	-0.0154 [0.0114]	0.0144* [0.00816]	0.000673 [0.0127]	-0.00184 [0.0143]	0.0108 [0.00862]	-0.0449 [0.0328]
WeatherEventXPdayAccess	0.0286** [0.01115]	0.0419** [0.0203]	0.00966 [0.0182]	0.0201 [0.0210]	0.0301 [0.0213]	-0.0202 [0.0128]	-0.00161 [0.0200]	0.0035 [0.0217]	-0.00635 [0.0169]	0.105* [0.0548]
PaydayAccess	-0.035 [0.0257]	-0.123*** [0.0384]	-0.0775** [0.0381]	-0.0316 [0.0429]	-0.135*** [0.0485]	0.0420** [0.0199]	-0.0486* [0.0265]	-0.0511 [0.0380]	0.0375 [0.0308]	-0.0569 [0.0899]
No. Obs	191,003	147,242	189,543	62,771	73,276	187,429	143,062	116,778	178,496	30,102
R-squared	0.373	0.244	0.484	0.381	0.247	0.323	0.164	0.141	0.256	0.084

This table presents results from empirical specification (2). WeatherEvent is a dummy variable equal to 1 if a household lives in a county that experienced a weather event in a month. Regressions include household-level controls (housing tenure, education level, race, age, family size, income class, and a cubic in household income), state-level controls (personal income growth, the log of personal income, and the log of house prices), county-level controls (the unemployment rate and employment growth) and state and year fixed effects. Standard errors are presented in brackets below the coefficient estimates and are clustered at the county level. The sample period is 1998 to 2010. ***, **, and * indicate 1 percent, 5 percent and 10 percent significance, respectively.

APPENDIX

Appendix 1. Variable Construction

The analysis relies on constructing estimates of the firm losses and previous-years' profits that were available to apply to the carryback policy, as well as the size of the firm tax refund received as a result of the policy.

1) Taxable income and tax rates:

A. I define taxable income as follows, with Compustat variable names in parenthesis:

$$\text{Taxable Income} = \text{Domestic Pretax Income (PIDOM)} - \text{Federal Deferred Taxes (TXDFED)} / \tau + \text{Extraordinary Items and Discontinued Operations (XIDO)} / (1 - \tau)$$

Here, $\tau = 0.35$ is assumed to be the marginal tax rate. Pretax Income equals Operating Income After Depreciation (OIADP) – Interest and Related Expenses (XINT) + Special Items (SPI) + Non-Operating Income (NOPI). When domestic pretax income and federal deferred taxes are missing, I use total Pretax Income (PI) and Deferred Income Taxes (TXDI). I replace any missing values for extraordinary items with zero.

B. I define the tax rate as follows:

$$\text{Tax Rate} = \text{Federal Income Taxes (TXFED)} / \text{Taxable Income}$$

If data on federal income taxes paid are missing, I replace missing values with Total Income Taxes (TXT) – Foreign Income Taxes (TXFO) – State Income Taxes (TXS) – Deferred Taxes (TXDI) – Other Income Taxes (TXO). I replace missing values for foreign income taxes, state income taxes, deferred taxes and other income taxes with zeros.

2) Losses to apply to the five-year carryback policy:

- A. For the 2002 policy period, I assume a firm would have applied any 2001 losses (negative taxable income) to the 2002 refund and any 2002 losses to the 2003 refund.
- B. For the 2009 policy period, firms were only allowed to apply either 2008 *or* 2009 losses to the five-year carryback.
- I estimate the potential tax refunds if the firm applied 2008 losses or applied 2009 losses to the five-year carryback. I assume that firms would have chosen the higher refund.
 - If I estimate that the firm would have applied the 2008 losses to the five-year carryback policy, losses that apply to the policy for the 2010 refund equal 2008 plus 2009 losses. I assume the firm would also have applied any 2009 losses to the standard two-year carryback policy in that year.
 - If I estimate that the firm applied the 2009 losses to the five-year carryback policy, losses that apply to the 2010 refund just equal the 2009 losses. Firms cannot receive any additional refund based on 2008 losses.

3) Previous-years' profits to apply to the policy:

- A. For each year in the five-year carryback window, if any year in the window had a loss— $year_{(t)}$ —I calculate if there were any adjustments for a two-year carryback during that time.
- a. If the firm had profits in $year_{(t-2)}$:
- If $profits_{(t-2)}$ were larger than the $loss_{(t)}$, I assume the firm received a refund for the $loss_{(t)}$. The $profits_{(t-2)}$ remaining for the five-year carryback policy equal $profits_{(t-2)}$.

minus the losses_(t). The firm has no more losses to apply to the two-year carryback.

- If profits_(t-2) were smaller than the loss, I assume the firm received a refund for the profits_(t-2). Profits_(t-2) remaining for the five-year carryback policy are zero. Losses_(t) that remain to apply to a refund equal losses_(t) minus profits_(t-2).

b. If the firm had profits_(t-1) and had losses_(t) remaining to apply for a carryback:

- If profits_(t-1) were larger than the remaining loss_(t), I assume the firm received a refund for the whole loss_(t). The profits_(t-1) remaining for the five-year carryback policy equal profits_(t-1) minus the loss_(t).
- If profits_(t-1) were smaller than the loss_(t), I assume the firm received a refund for the profit_(t-1). Profits_(t-1) remaining for the five-year carryback policy are zero.

B. For profits that applied to the 2002 tax refund, previous-years' profits that were potentially available for the tax refund in 2002 equal the sum of profits from 1996 to 2001 that remained on the firm's books.

C. For profits that applied to the 2003 tax refund, I assume that any profits applied to the 2002 tax refund were not available for the 2003 tax refund.

- a. If the 2001 loss was larger than the sum of profits from 1996 to 2000 that could apply to the policy, previous-years' profits that apply to the 2003 refund equal zero.
- b. If the 2001 loss was smaller than the 1996 profit that could be applied to the refund, previous-years' profits that applied to the 2003 refund equal the sum of profits from 1997 to 2000.
- c. If the 2001 loss was larger than the 1996 profit that could be applied to the refund but smaller than the total sum of profits from 1996 to 2000, previous-years' profits that

applied to the 2003 refund equaled the sum of profits from 1996 to 2000 minus the 2001 loss.

d. If 2001 was a profit year, the previous-years' profits that could apply to the 2003 refund equal the sum of profits from 1997 to 2001.

D. For profits that applied to the 2010 tax refund, if I calculate that firms apply 2008 losses to the five-year carryback policy, profits that apply equal the sum of profits from 2003 to 2007. If I calculate that firms apply 2009 losses to the five-year carryback policy, profits that apply to the policy equal the sum of profits from 2004 to 2008.

Tax Refunds:

To calculate the tax refund each firm would have received, I assume that firms receive a refund based on the 5th year of the window first, then the 4th year of the window, then the 3rd year etc... Note that for the 2009 policy period, only 50 percent of profits in the 5th year of the window could apply to the policy.

Starting with the last year of the window, year_(t-5):

A. If the firm's policy losses_(t) are larger than the profits in year_(t-5), the tax refund equals that year's tax rate times profits_(t-5) available. The losses_(t) that now can apply to a refund equals losses_(t) minus profits_(t-5). Profits_(t-5) remaining to apply for another carryback equal zero.

B. If the firm's policy losses are smaller than the profits in that year, the tax refund equals the year's tax rate times the losses_(t) available. The firm has now exhausted the losses available to apply to the policy. Profits_(t-5) remaining to apply for another carryback equal profits_(t-5) minus losses_(t).

C. I repeat the algorithm for the other 4 years in the window.

Appendix 2. Variable Definitions

I source financial and accounting data from the Compustat fundamental annual database and the CRSP daily and monthly annual update databases. I source data on firm credit ratings from the Capital IQ S&P credit ratings database. I source data on analyst forecast dispersion from I/B/E/S and data on firm marginal tax rates are from John Graham. Variables definitions used in the analysis are as follows:

$$\text{Investment} = \text{CAPXV} - \text{SPPE}$$

$$\text{Change in cash} = \text{CH} - \text{CH}_{t-1}$$

$$\text{Change in total debt} = (\text{DLC} + \text{DLTT}) - (\text{DLC}_{t-1} + \text{DLTT}_{t-1})$$

$$\text{Change in long-term debt} = \text{DLTT} - \text{DLTT}_{t-1}$$

$$\text{Change in short-term debt} = \text{DLC} - \text{DLC}_{t-1}$$

$$\text{Payout} = \text{DVC} + \text{PRSTKC}$$

$$\text{Change in short-term investments} = \text{IVST} - \text{IVST}_{t-1}$$

$$\text{Change in investments} = \text{IVCH}$$

$$\text{Acquisitions} = \text{AQC}$$

$$\text{Change in Employment} = \text{EMP} - \text{EMP}_{t-1}$$

$$\text{Altman's z-score} = 3.3 \cdot \text{EBIT}/\text{AT} + 1.0 \cdot \text{SALE}/\text{AT} + 1.4 \cdot \text{RE}/\text{AT} + 1.2 \cdot \text{WCAP}/\text{AT}$$

$$\begin{aligned} \text{Ohlson's o-score} = & -1.32 - 0.407 \cdot \ln(\text{AT}) + 6.03 \cdot (\text{LT}/\text{AT}) - 1.43 \cdot (\text{ACT} - \text{LCT})/\text{AT} \\ & + 0.0757 \cdot (\text{LCT}/\text{ACT}) - 2.37 \cdot \text{NI}/\text{AT} - 1.83 \cdot (\text{PI} + \text{DP})/\text{LT} + 0.285 \cdot 1 \cdot [(\text{NI}_{t-1} < 0 \ \& \ \text{NI}_{t-2} < 0)] - \\ & 1.72 \cdot (\text{LT} > \text{AT}) - 0.521 \cdot (\text{NI} - \text{NI}_{t-1})/(|\text{NI}| + |\text{NI}_{t-1}|) \end{aligned}$$

Distance to default is calculated as the naive measure in Bharath and Shumway (2008):

$N(-\text{DD})$, where

$$\text{DD} = \frac{\ln\left(\frac{E+F}{F}\right) + (r_{it-1} - \frac{\sigma_V^2}{2})T}{\sigma_V \sqrt{T}}$$

$$\sigma_V = \left(\frac{E}{E+F}\right) \sigma_E + \left(\frac{F}{E+F}\right) (0.05 + 0.25 \sigma_E)$$

E is the market value of equity ($\text{PRC} \cdot \text{SHROUT}$ in CRSP); F is the face value of debt, defined as $\text{DLC} + 0.5 \cdot \text{DLTT}$; r_{it-1} is the firm's buy-and-hold stock return over the previous year; T is the time to maturity (one or two years); σ_E is the standard deviation of the firm's stock price over the previous year; and σ_V is the approximation to the total volatility of each firm.

S&P credit rating upgrade is a dummy variable equal to one if a firm experiences a ratings upgrade on long-term debt within a given period and zero otherwise. The S&P credit rating variable used is SPLTICRM.

S&P credit rating downgrade is a dummy variable equal to one if a firm experiences a ratings downgrade on long-term debt within a given period and zero otherwise. The S&P credit rating variable used is SPLTICRM.

Bankruptcy or liquidation is a dummy variable equal to one if a firm goes through bankruptcy or liquidation in a given period (Compustat DLRSN value of 2 or 3) and zero otherwise.

$$\text{ROA} = \text{OIBDP}/\text{AT}$$

$$\text{Tobin's } q = (\text{AT} + \text{PRCC}_F \cdot \text{CSHO} - (\text{SEQ} + \text{TXDITC} - \text{PSTK}))/\text{AT}$$

$$\text{Cash Flow/Assets} = (\text{IB} + \text{DP})/\text{AT}$$

$$\text{Ln(Assets)} = \ln(\text{AT})$$

$$\text{Leverage} = (\text{DLC} + \text{DLTT})/(\text{DLC} + \text{DLTT} + \text{PRCC}_F \cdot \text{CSHO})$$

$$\text{Sales/Assets} = \text{SALE}/\text{AT}$$

Marginal Tax Rate is defined as MTR_BEFINT from John Graham's marginal tax rate file. When a data point is missing, I use the algorithm to estimate the book stimulated marginal tax rate from Graham and Mills (2008): $0.331 - 0.075 \cdot 1 \cdot [\text{TXFED}/\text{PIDOM} < 0.1] - 0.012 \cdot 1 \cdot [\text{TLCF} > 0] - 0.106 \cdot 1 \cdot [\text{PI} < 0] + 0.037 \cdot 1 \cdot [|\text{PIFO}/\text{PI}| > 0.05]$. If TXFED/PIDOM is missing, I substitute TXT/PI.

Appendix 3. Additional Figures and Tables

Figure A1: Distribution of firms in 2001 and in 2002 that qualified for the tax refund policy around the kink point V^*

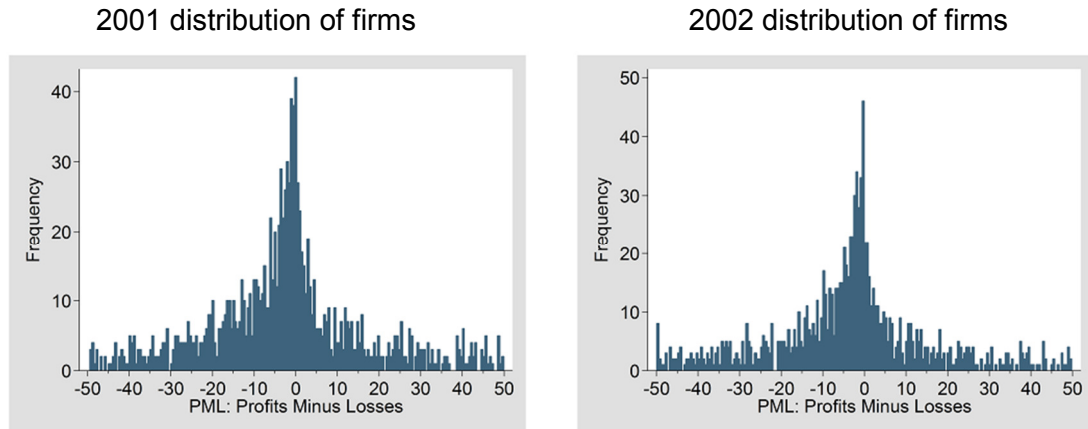


Figure A2: Distribution of firms that qualified for the 2002 policy around the kink point V^* , by the duration of investment goods by industry

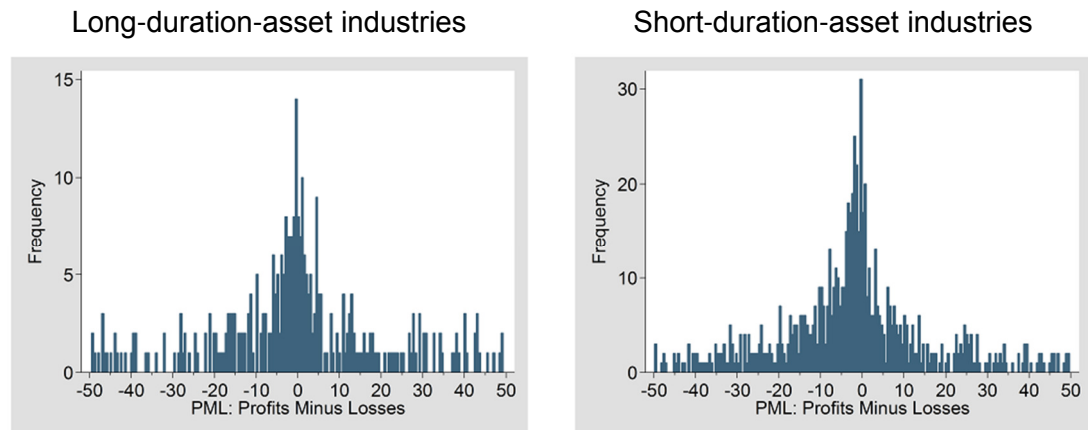


Table A1: Sharp RKD

	2002 Policy	2009 Policy			
	Investment	Cash Ch.	Change in Cash (2011)	Change in L.T. Debt (2011)	Change in L.T. Debt (2010 & 2011)
Dependent variable =	(1)	(2)	(3)	(4)	(5)
Change in slope (β_1)	0.145** [0.0642] (0.0295)	0.309 [0.186] (0.105)	-0.494** [0.209] (0.0231)	-0.390** [0.187] (0.0435)	-0.444** [0.191] (0.0254)
Controls	+	+	+	+	+
Industry F.E.	+	+	+	+	+
Observations	3,337	1,496	1,355	1,355	1,355
R-squared	0.403	0.254	0.104	0.094	0.120

This table presents the coefficient β_1 from empirical specification (1) and estimates the change in slope of the outcome variable around the kink point. The coefficient estimate divided by 0.35 (the highest marginal tax rate) equals a "sharp" RKD estimate of the effect of the tax refund on firm outcomes. Column 1 reports results for investment as the dependent variable in the 2002 policy period and columns 2 through 5 report results for the following dependent variables in the 2009 policy period: change in cash in 2010, change in cash in 2011, change in long-term debt in 2011 and the cumulative 2-year change in long-term debt over 2010 and 2011. Regressions include a second-order polynomial in the assignment variable "V", industry fixed effects at the Fama-French 48 level, and the pre-treatment controls Tobin's q, ROA, cash flow/assets, sales/assets, leverage, the marginal tax rate, $\ln(\text{assets})$, and a quadratic in stimulus losses. Standard errors are clustered at the industry level and are reported in brackets. P-values are reported in parentheses. ***, ** and * indicate levels of 1 percent, 5 percent, and 10 percent significance, respectively.

Table A2: Robustness to Polynomial Choice

Polynomial Order:					
	Second Stage:	Two		Three	
	First Stage:	Two	One	Three	One
		(1)	(2)	(3)	(4)
2002 Policy					
Investment		0.403*** [0.155] (0.00953)	0.467*** [0.181] (0.00980)	0.386** [0.186] (0.0379)	0.431** [0.176] (0.0145)
2009 Policy					
Change in Cash (2010)		0.958* [0.576] (0.0963)	0.461 [0.730] (0.528)	0.895 [0.550] (0.104)	0.668 [0.532] (0.210)
Change in Cash (2011)		-1.260** [0.626] (0.0440)	-1.165 [0.741] (0.116)	-1.183* [0.647] (0.0674)	-1.186* [0.680] (0.0810)
Change in L.T. Debt (2011)		-1.542* [0.828] (0.0626)	-0.841 [0.736] (0.253)	-1.677** [0.842] (0.0463)	-1.522* [0.818] (0.0627)
Change in L.T. Debt (2010 & 2011)		-1.361** [0.622] (0.0285)	-0.473 [0.906] (0.602)	-1.515** [0.717] (0.0345)	-1.540** [0.725] (0.0337)
Controls		+	+	+	+
Industry F.E.		+	+	+	+

This table presents the coefficient β_1 from empirical specification (2), the second-stage regression, and estimates the effect of the tax refund on potential uses of the funds with varying orders of polynomials in "V" in the second-stage and first-stage regressions. Each panel reflects a separate regression. Dependent variables are reported in rows. Column 1 reports results for the preferred specification: a second-order polynomial in V in the second stage $[\sum_{p=1}^2 (V_{it-1} - V^*)^p]$ and a second-order polynomial in the excluded instrument $[\sum_{p=1}^2 D \cdot (V_{it-1} - V^*)^p]$ in the first stage. Column 2 reports results for a second-order polynomial in V in the second-stage and a first-order polynomial in the excluded instrument in the first-stage. Columns 3 and 4 report results for a third order polynomial in V in the second stage and a third- and first-order polynomial in the excluded instrument in the first stage, respectively. Regressions include industry fixed effects at the Fama-French 48 level, and the pre-treatment controls Tobin's q, ROA, cash flow/assets, sales/assets, leverage, the marginal tax rate, $\ln(\text{assets})$, and a quadratic in stimulus losses. Standard errors are clustered at the Fama-French 48 industry level and are reported in brackets. P-values are reported in parenthesis. ***, **, and * indicate levels of 1 percent, 5 percent, and 10 percent significance, respectively.

Table A3: Robustness to Varying Bandwidths

Dependent variable =	2002 Policy	2009 Policy			
	Investment	Change in (2010)	Change in Cash (2011)	Change in L.T. Debt (2011)	Change in L.T. Debt (2010 & 2011)
	(1)	(2)	(3)	(4)	(5)
Bandwidth Range:					
Full Sample	0.403*** [0.155]	0.958* [0.576]	-1.542* [0.828]	-1.260** [0.626]	-1.361** [0.622]
[-2750, 2750]	—	0.856* [0.472]	-1.706** [0.797]	-1.707** [0.710]	-2.072** [0.846]
[-2500, 2500]	—	0.512 [0.496]	-1.204 [0.747]	-1.413** [0.683]	-1.338* [0.799]
[-2250, 2250]	0.474*** [0.173]	0.134 [0.569]	-0.852 [0.758]	-1.844** [0.797]	-2.201** [0.977]
[-2000, 2000]	0.425* [0.257]	0.11 [0.619]	-0.958 [0.890]	-2.072*** [0.766]	-2.823*** [1.089]
[-1750, 1750]	0.371 [0.267]	0.494 [0.764]	-1.543 [0.952]	-1.362 [0.946]	-1.53 [0.994]
[-1500, 1500]	0.485** [0.217]	-0.0987 [0.882]	-1.331 [1.021]	-1.652* [0.854]	-2.127** [1.080]
[-1250, 1250]	0.915*** [0.349]	0.852 [0.816]	-1.273 [1.166]	-1.611 [1.078]	-0.355 [1.514]
[-1000, 1000]	0.662** [0.331]	1.346 [0.863]	0.588 [1.709]	-1.668 [1.433]	0.162 [1.887]
[-750, 750]	0.915** [0.377]	0.96 [0.702]	0.114 [1.139]	-0.199 [1.270]	0.218 [1.866]
[-500, 500]	1.772*** [0.653]	1.344 [1.062]	-0.946 [1.811]	-1.521* [0.876]	-1.104 [2.092]
Controls	+	+	+	+	+
Industry F.E.	+	+	+	+	+

This table presents the coefficient β_1 from empirical specification (2), the second-stage regression, and estimates the effect of the tax refund on potential uses of the funds with varying bandwidths of the regression window. Each panel reflects a separate regression. Column 1 reports results for investment as the dependent variable in the 2002 policy period and columns 2 through 5 report results for the following dependent variables in the 2009 policy period: change in cash in 2010, change in cash in 2011, change in long-term debt in 2011 and the cumulative 2-year change in long-term debt over 2010 and 2011. Regressions include a second-order polynomial in the assignment variable "V", industry fixed effects at the Fama-French 48 level, and the pre-treatment controls Tobin's q, ROA, cash flow/assets, sales/assets, leverage, the marginal tax rate, $\ln(\text{assets})$, and a quadratic in stimulus losses. Standard errors are clustered at the industry level and are reported in brackets. ***, ** and * indicate levels of 1 percent, 5 percent, and 10 percent significance, respectively.

Table A4: Excluding Hard-Hit Industries

Dependent variable =	2002 Policy	2009 Policy			
	(Ex. Air & Telecom)	(Ex. Homebuilders)			
	Investment	Change in Cash (2010)	Change in Cash (2011)	Change in L.T. Debt (2011)	Change in L.T. Debt (2010 & 2011)
	(1)	(2)	(3)	(4)	(5)
Tax Refund	0.505* [0.268] (0.0592)	1.036 [0.664] (0.119)	-1.48 [0.954] (0.121)	-1.562** [0.713] (0.0286)	-1.685** [0.687] (0.0141)
Controls	+	+	+	+	+
Industry F.E.	+	+	+	+	+
Observations	3,213	1,449	1,311	1,311	1,311
R-squared	0.400	0.259	0.028	0.076	0.111

This table presents the coefficient β_1 from empirical specification (2), the second-stage regression, and estimates the effect of the tax refund on potential uses of the funds. Column 1 reports results for investment as the dependent variable in the 2002 policy period and columns 2 through 5 report results for the following dependent variables in the 2009 policy period: change in cash in 2010, change in cash in 2011, change in long-term debt in 2011 and the cumulative 2-year change in long-term debt over 2010 and 2011. Column 1 excludes firms in Fama-French industries 23 and 32: aircraft and communications. Columns 2 to 5 excludes firms in Fama-French industries 17 and 18: construction materials and construction. Regressions include a second-order polynomial in the assignment variable "V", industry fixed effects at the Fama-French 48 level, and the pre-treatment controls Tobin's q, ROA, cash flow/assets, sales/assets, leverage, market-to-book, the marginal tax rate, $\ln(\text{assets})$, and a quadratic in stimulus losses. Standard errors are clustered at the industry level and are reported in brackets. P-values are reported in parenthesis. ***, ** and * indicate levels of 1 percent, 5 percent, and 10 percent significance, respectively.

Table A5: Tax Refund Allocation in the 2002 Policy Period**Panel A: Year After Tax Refund Receipt (2003 and 2004)**

Dependent variable =	Investment	Change in Cash	Change in Debt	Payout	Other Uses	Total
	(1)	(2)	(3)	(4)	(5)	(6)
Tax Refund	0.182 [0.449]	0.613 [0.484]	1.128 [1.064]	-0.0898 [0.186]	0.333 [0.668]	-0.0905 [1.085]
Controls	+	+	+	+	+	+
Industry F.E.	+	+	+	+	+	+
Observations	2,952	2,952	2,952	2,952	2,952	2,952
R-squared	0.33	0.00	0.01	0.11	0.13	0.25

Panel B: Two-Year Total Effect After Tax Refund Receipt

Dependent variable =	Investment	Change in Cash	Change in Debt	Payout	Other Uses	Total
Tax Refund	0.425 [0.642]	0.415 [0.523]	0.467 [0.892]	-0.146 [0.336]	0.771 [1.065]	0.997 [1.211]
Controls	+	+	+	+	+	+
Industry F.E.	+	+	+	+	+	+
Observations	2,952	2,952	2,952	2,952	2,952	2,952
R-squared	0.39	0.05	0.08	0.15	0.11	0.31

This table presents the coefficient β_1 from empirical specification (2), the second-stage regression, and estimates the effect of the tax refund on potential uses of the funds in the 2002 policy period. Panel A reports estimates for the year following tax refund receipt (2003 and 2004) and Panel B reports estimates for the cumulative use of funds the two years after tax refund receipt. Other uses are acquisitions, change in short-term investments and change in investments. Total is the sum of all uses listed in columns 1 to 5. Regressions include a second-order polynomial in the assignment variable "V", industry fixed effects at the Fama-French 48 level, and the pre-treatment controls Tobin's q, ROA, cash flow/assets, sales/assets, leverage, the marginal tax rate, $\ln(\text{assets})$, and a quadratic in stimulus losses. Standard errors are clustered at the industry level and are reported in brackets. ***, **, and * indicate levels of 1 percent, 5 percent, and 10 percent significance, respectively.

**Table A6: Investment, Financial Constraints, and Investment Opportunities:
Effects of the Tax Refund on Investment in the 2009 Policy Period**

Panel A: Financial Constraints

Dependent Variable = Investment

Financially Constrained?	KZ Index		Payout		DFF	
	Yes	No	Yes	No	Yes	No
	(1)	(2)	(3)	(4)	(5)	(6)
Tax Refund	-0.978 [0.880]	0.298 [0.360]	-1.157 [0.794]	-0.324 [0.409]	-1.065 [0.767]	-0.229 [0.333]
Controls	+	+	+	+	+	+
Industry F.E.	+	+	+	+	+	+
Observations	355	356	939	557	1,087	406
R-squared	0.44	0.50	0.44	0.46	0.41	0.52

Panel B: Investment Opportunities

Dependent Variable = Investment

High Investment Opportunities	Tobin's Q		Multi-national	
	High	Low	Domestic	
	Yes	No	Yes	No
	(1)	(2)	(3)	(4)
Tax Refund	-0.702 [0.799]	-3.944 [3.277]	-0.581 [0.580]	-0.341 [0.274]
Controls	+	+	+	+
Industry F.E.	+	+	+	+
Observations	373	374	551	942
R-squared	0.39	0.00	0.47	0.42

This table presents the coefficient β_1 from empirical specification (2), the second-stage regression, and estimates the effect of the tax refund on investment. Panel A restricts the sample by three measures of firm financial constraints. Columns 1 and 2 show results for subsamples of firms in the top and bottom quartiles of the Kaplan-Zingales (1997) index, respectively. Columns 3 and 4 show results for subsamples of firms with zero and non-zero dividend issuance and stock repurchases, respectively. Columns 5 and 6 show results for subsamples of firms for which total payouts to operating income is and is not less than or equal to zero, respectively, as in Dharmapala, Foley, and Forbes (2011). Panel B restricts the sample by two measures of firm investment opportunities. Columns 1 and 2 show results for subsamples of firms in the top and bottom quartiles of Tobin's q, respectively. Columns 3 and 4 show results for firms with substantial foreign activity (foreign pre-tax income greater than 5 percent of total pre-tax income in absolute value) and domestic firms, respectively. All financial constraint and investment opportunity measures are calculated for the year prior to tax refund receipt. Regressions include a second-order polynomial in the assignment variable "V", industry fixed effects at the Fama-French 48 level, and the pre-treatment controls Tobin's q, ROA, cash flow/assets, sales/assets, leverage, the marginal tax rate, $\ln(\text{assets})$, and a quadratic in stimulus losses. Standard errors are clustered at the industry level and are reported in brackets. ***, ** and * indicate levels of 1 percent, 5 percent, and 10 percent significance, respectively.

Appendix 3. Income Classes in the Consumer Expenditure Survey

Prior to 2004, the Consumer Expenditure Survey included only household income as directly reported. Due to the large share of non-response to income questions, the CE currently uses income imputation to fill in income blanks. In 2004 and 2005, the CE only published imputed data, and starting in 2006, the CE started publishing both the imputed income data and the reported data.

For this study, in order to maintain consistency across the sample period, I only include observations for complete income reporters for the sample years 1998-2003 and 2006-2010. I define complete income reporters as households that report non-zero income in at least one of the following categories: wages and salaries; unemployment compensation; income from nonfarm business, partnership or professional practice; farm income; Social Security payments or Railroad Retirement income; Supplemental Security Income; welfare income; and pension income. Since BLS only reports imputed income for 2004 and 2005, in those years, I exclude households for which BLS reported that all of the income categories above had been imputed because the data had been invalid blanks (data flags 2 or 5). To separate households into income classes, I use total, before-tax income (code `fincbefx` for 1998-2003 and `fincbefm` for 2004 and 2005).

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